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LECTURE I.

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Lying beneath the cuticle is the dermis. This is divisible into two strata, viz: the papillary, and the corion. The former is of less consistence than the latter, and is arranged in the form of minute papillæ, in which is supposed to reside, *par excellence*, the sense of touch, or feeling. The latter is firmer, areolar in its texture, containing white inelastic, commingled with yellow elastic fibres, the latter, under the influence of cold, and sometimes mental emotions, undergoing sensible contractions, giving rise to the appearance vulgarly known as goose-skin.

In the areolæ of the corion are found cryptal, cœcal, or minute blind cavities in which an unctuous matter is secreted. These are the sebaceous glands or follicles. Beneath the integument, in the subdermoid tissue, are found small convoluted tubuli, or congeries of globular sacs which furnish the special secretion constituting the insensible and sensible perspiration. These are the sudoriparous glands. The tubuli unite to form ducts which, pursuing a serpentine course, pass through the dermis, and terminate by open pores upon the surface of the epidermis. The perspiratory tubuli and glands, although separately considered quite insignificant in size, constitute in their aggregate over the entire surface of the body an immense secreting surface. It is estimated by Mr. WILSON that, thrown into a single canal, the united tubuli would form a duct forty-eight thousand six hundred yards in length, or nearly twenty-eight miles! The average daily loss of matter by means of the secretory function of this glandular element of the cutaneous surface, is estimated to be about two pounds and a half. A large proportion of the matter eliminated is water. Carbonic acid is also thrown off—and a certain amount of nitrogen. The proportion of solids to the whole amount of perspiration is small, not exceeding a few scruples in the twenty-four hours. Still, as a depurative organ, the skin possesses great importance in the animal economy.

The skin is richly supplied with blood-vessels, absorbents and nerves. It therefore has a high degree of vital activity, and is the most sensitive of the compound structures.

The hair and nails are called appendages of the skin. Their composition is similar to that of the epidermis, and they may be considered, in fact, component parts of that portion of the cutaneous system.

To describe minutely the anatomical structure, mode of development and functions of the different cutaneous elements which have just been mentioned, does not, of course, fall within my province. These different elements, however, are, to some extent, individually the seat of disease, and they contribute, separately, to furnish distinctive characters pertaining to different cutaneous affections; hence, we shall have occasion to refer to them in treating of this department of special pathology.

When we consider in connection with the complexity of the anatomical composition of the skin, the extent of its expanded surface, its exposure to the direct action of external morbid influences, its high grade of vital activity, its great nervous susceptibility and its important function as an excretory organ, we should naturally infer that it must be liable to various forms of morbid action. Such is the fact. The proportion of cutaneous affections among the variety of diseases which come under the observation of the practitioner of medicine is considerable, and they are presented under a great diversity of aspects. It would also be a natural inference from these circumstances that this class of diseases would occupy not a small share of the attention of medical practitioners, and fill a commensurate space in medical literature. When it is farther considered that these affections, being situated externally, are open to direct examination and study by means of the senses, in this respect offering great advantages over those which, concealed from inspection during life, can only be investigated by means of their signs and symptoms, it might reasonably be expected that our scientific and practical knowledge of them would be more complete, and that it would be more generally diffused among the profession than in the instance of almost any other class in the nosology. The state of the case, however, is the very reverse of this. There are few diseases in the whole nosological catalogue of which science possesses so little satisfactory information, and which are so generally neglected by practitioners, as those affecting the skin. There is scarcely any other class of which so little has been written. The whole bibliography of skin diseases will not embrace many treatises. Systematic writers on practical medicine are accustomed to pass them over with very

cursory notice. Formal instruction relating to them rarely enters into the curriculum of instruction in our medical schools; and the practitioner seldom feels mortification in betraying utter ignorance of what has been accomplished in this department of practical medicine. I believe that I do not do injustice to the medical profession when I say that the majority of its members pay little, or no attention to the differential diagnosis of cutaneous diseases. Therapeutical measures are therefore indiscriminately employed, without reference to distinctions, which, in this point of view, are of practical importance. Hence it is that so few have contributed to the general stock of knowledge; and hence, also, is it, in a measure, that the skin affections hold a preëminent station among the *opprobria* of medicine. Of all diseases these are perhaps most apt to persist in spite of remedies, and they are therefore a profitable field of revenue for the charlatan, and the vender of secret nostrums. The inquiry will not fail to arise in your minds, whence this strange neglect of a class of affections which all must admit are by no means least in importance? We shall be better prepared to meet this question hereafter, but I will say here that although it arises, in part, from difficulties inherent in the study, it is attributable, in no small measure, to the fact that the study has been rendered gratuitously complicated. I will enlarge upon this point somewhat presently, and in this connection will therefore only observe, that if the diseases of this class were to be presented with that simplicity which is not only admissible, but fully compatible with the attainment of all important scientific and practical ends, there would be no excuse for the prevalent neglect of which I have spoken, but, on the other hand, this department of special pathology would present to the student and practitioner peculiar attractions.

The great variety in the external characters and other circumstances pertaining to cutaneous diseases, is calculated to give rise to an impression that they are too numerous, diversified and eccentric for either scientific analysis, or practical discrimination. But without some classification based upon resemblances reliable, and recognisable, it is evident their investigation must be exceedingly imperfect and unsatisfactory. The study of this class of diseases may be said to have first become truly a department of medical

science when its subjects were first generalized and distributed into a systematic arrangement. This was the origin of dermatology, as the science treating of cutaneous diseases is sometimes styled. The first successful architect of this science was PLENCA, or PLENCK, a Hungarian physician. To him is due the credit of first forming a classification founded on elementary characters appertaining to the various forms of cutaneous affections. Prior to his time they had been distributed by some writers according to the portion of the body affected—the head, trunk and extremities; and by others they were arranged according to their supposed causes, which were too little known to serve as a permanent basis for that purpose. PLENCK published his classification in 1776 (a date which should be easily remembered by the American medical student). In 1798, WILLAN, an English writer, published a treatise in which he adopted PLENCK's classification, with some important modifications. Since the time of the latter publication, the classification has been known as the *Willanean arrangement*, and has been generally accepted, with additional improvement, by the profession in all countries. The classification of PLENCK and WILLAN is founded on resemblances in external characters determinable in the early stages of cutaneous diseases. Notwithstanding these diseases after some continuance present widely different appearances, it was ascertained that in their primitive, elementary forms they are resolvable into a few groups. PLENCK established fourteen classes, or orders. WILLAN reduced this number to eight, which are still recognised by most writers, although it is probable a still greater reduction is admissible. Attempts have been made to substitute other methods of arrangement for the Willanean. The distinguished French dermatologist, ALIBERT, and one of his successors at the St. Louis hospital, CAZENAVE, have made such attempts. But perhaps the ablest effort of this kind is that of an English author, Mr. ERASMUS WILSON. Mr. WILSON endeavors to institute divisions after distinctions founded on the particular anatomical elements of the skin affected, and on supposed intrinsic differences in the nature, as well as seat of the morbid action. Were this plan practicable it would undoubtedly be the most scientific and satisfactory. But we are not, with our present knowledge, prepared to decide on such distinctions; and

there is great risk of substituting speculations for facts in distributing the different affections after this plan. We could not discuss the merits of the *Wilsonian* method, as contrasted with the *Willanean*, till the latter has been considered more fully; but it will hardly be advisable to engage in such a discussion. The system of WILLAN appears to my mind far more simple, and in every respect preferable to that of WILSON, or any other so called natural arrangement. It is depreciatingly termed an artificial system, and so to some extent it undoubtedly is. But it is based on positive anatomico-pathological distinctions, which are visible, generally ascertained without great difficulty, involving no hypothetical assumptions, and, practically, it is convenient and trustworthy. The fundamental principles on which it rests possess so many advantages that I do not believe it will very soon, if ever, be supplanted, although, as already remarked, it admits of greater simplification and improvement than it has yet received.

The *eight* groups, or orders of the Willanean system are as follows:

First group or order. Exanthematæ. Rashes. Most of the affections embraced in this class we have considered under the head of febrile diseases. They constitute the eruptive fevers. They are characterized, each by a peculiar eruption, and their diagnostic criteria consist, in a great measure, of the characters pertaining to this eruption; but the cutaneous affection is confessedly secondary, presenting the external manifestations only of a general disease, viz: *Fever.* We have every reason to believe, it is true, that other groups of skin diseases are equally related to some internal derangement, but the connection has not been so positively established, and too little is known respecting the primary disorder in other classes, to admit of their entering into a system of classification. Treating of the exanthemata in another part of my didactic course, viz: in connection with fevers, I shall not take them up under the head of cutaneous diseases.

Second group or order. Vesiculæ. Vesicles. The characteristics of this class are the presence of small elevations of the cuticle, containing a fluid, at first transparent, resembling serum or water. In most instances, after a time, the contained fluid be-

comes opaque, milky, puruloid or distinctly purulent. The liquid may be resorbed, or discharged by rupture of the vesicles, forming crusts, and sometimes scabs, more or less intermingled with scurf or epidermic scales. Vesicles are always minute in size, each holding a small quantity of serum or lymph.

Third group or order. Bullæ. Blebs. This class consists of tumors caused by the elevation of the cuticle from an effusion of serum or sero-purulent fluid, between the epidermis and corion. Their size varies from that of a pea, to an egg. The type of this group is a blister. They resemble, in every particular, excepting size, vesicles; and the two might appropriately enough be embraced in the same order, as has been done by some late writers, (NELLIGAN, CAZENAVE, and BENNETT). Practically, however, this does not simplify the subject, since it is as easy, if not more so, to recollect the bullæ as a distinct order, than as different genera of another group.

Fourth group or order. Pustulæ. Pustules. These terms signify pimples containing pus. An elevation of the cuticle containing, *from its development*, a purulent fluid, comes in this class. Pustules rarely end by resorption, but the pus escapes and concretes, forming scabs. Vesicles frequently, during their progress contain purulent fluid, but it is preceded by serum or lymph. Pustules, from the earliest period that the nature of the fluid can be ascertained, are found to contain pus; and herein lies the essential point of distinction.

Fifth group or order. Papulæ. Pimples. Pimples are small solid elevations containing no fluid. By their solidity and want of fluid contents, they are sufficiently distinguished from vesicles and pustules, with which alone they can be confounded.

Sixth group or order. Squamæ. Scales. The distinguishing characters of this class are thickened, laminated, epidermic scales, which are exfoliated, and successively renewed in greater or less abundance. These can only be confounded with the crusts formed by the desiccation of the effused serum or lymph of ruptured vesicles; but the latter are preceded by a liquid exudation, while the former are, from the first, dry scales, produced by morbid action of the epidermic cells.

Seventh group or order. Maculæ. Spots. This is a class not

having much practical importance, embracing affections some of which can hardly be considered forms of disease, and in all cases easily distinguished from the diseases belonging to the other classes. They are seated in the rete mucosum, or are occasioned by effusion of blood in minute spots.

Eighth group or order. Tuberculæ. Tubercles. Tubercles, to quote the definition of RAYER, are "small, solid, circumscribed, indurated and enduring tumors, which, after continuing some months, often for several years, almost uniformly end by becoming changed in their nature and falling into a state of ulceration." From this brief description it is plain they need seldom, if ever, be confounded with the diseases belonging to other classes. In general, they are of infrequent occurrence save in tropical climates. The forms occasionally presented with us are treated of in the department of surgery under the head of tumors. For this reason I shall not take up the affections falling under this class.

From the foregoing eight groups or orders, eliminating two, viz: Exanthematæ and tuberculæ, there will remain six to be considered respectively, viz: Vesiculæ, Bullæ, Pustulæ, Papulæ, Squamæ and Maculæ. I shall take them up successively in the order just enumerated.

From the brief notice of the distinctive features of each of the several classes just enumerated, you will, I think, perceive that, so far, the arrangement is sufficiently simple, and that to determine in practice to which of the classes individual cases belong cannot generally be a very difficult undertaking. I shall present the characteristic circumstances appertaining to each class at greater length as I take up the respective groups in succession. The object in already referring to the more prominent of the distinguishing features is, that you may form an idea of the different classes collectively, before considering each class separately.

These several groups or orders are subdivided into genera and species. Here the classifications of WILLAN, and of all writers who have cultivated dermatology as a speciality, present a complexity which gives to the study a formidable and forbidding aspect. It is owing to this, in a great measure, that the subject is so generally neglected. It is clear to my mind that there is an over refinement in the minute subdivisions, and the technical

distinctions of WILLAN, and most other dermatologists; and inasmuch as upon this point it hinges, whether the medical student will decide to make himself master of the subject, or turn away from it as an impracticable branch of medical education, I may be excused for dwelling a little upon it. While I do not doubt that the study of diseases of the skin, like other medical specialties, affords ample range for exclusive industry, and admitting that to prosecute this department of practical medicine in all its minutiæ, ordinary facilities of observation are inadequate, I wish nevertheless to assume the position that, with such a proportionate degree of attention as will not necessarily compromise due proficiency in other branches of equal claims, and with the opportunities accessible in all large towns, a very satisfactory degree of acquirement, and of practical skill in diagnosis are attainable. When the student or practitioner opens a voluminous treatise, like that of RAYER, for example, devoted entirely to skin diseases, he is apt to be repelled by the apparent magnitude of the subject. When he considers, also, that exemplifications of the various forms of disease in private practice, or general hospitals, are presented only occasionally, and can only be studied with intervals of more or less duration between their occurrence, he is apt to become discouraged, and imagine that it is in vain to make any effort for personal qualification in this department of medical art, without the advantages of hospitals or dispensaries instituted with especial reference to cutaneous diseases. The very great facilities afforded by such institutions (which exist only in some of the larger metropolitan towns, like Paris) are sufficiently obvious. Very much time and labor are saved by having at the same moment before us illustrations of every variety of the subject which a scientific pursuit embraces. But for many, if not most persons these advantages are not available. The question, then, is, not whether they would be desirable, but whether they are absolutely essential. Shall the subject be neglected because these valuable opportunities are not to be obtained; or may they be dispensed with, and success secured by improving the comparatively few facilities which are available? This is the true question. And my answer is, that with means accessible to all, and with such an amount of exertion as may be bestowed without neglect of other matters, one may qual-

ify himself to understand and treat with satisfaction this division of diseases. It may be said of this subject, as of another branch of practical medicine, I refer to the physical exploration of the chest, that the want of a proper appreciation of the fewness and simplicity of the rules which are really of much practical consequence, constitutes the greatest obstacle to entering on the pursuit. The grand division of skin diseases into groups, or orders, as has been seen, is sufficiently intelligible. A very little study and observation are adequate to the mastery of the subject to that extent. The subdivisions into genera and species are, for ordinary practical purposes, needlessly numerous, especially the multiplication of species. They are refinements which are very well in a purely scientific point of view, but it is not necessary that the student or practitioner shall retain them all in his memory, for the usual duties of medical practice. Let him be prepared to refer different affections to their appropriate orders; let him be conversant with the more prominent generic distinctions, and let him have a general knowledge of those specific differences which are most liable to occur, and he is abundantly qualified to recognise and treat individual cases with a satisfactory degree of success.

The nomenclature of Willan's system is a great evil. The names are pedantic, often barbarous, devoid both of euphony, and of that significance which assists the memory by striking associations. The system admits of great improvement in this respect, but the terms have become so interwoven in medical literature, that it would not be easy to introduce others more simple and appropriate. A great number of them, however, may be practically disregarded without any scruples. I shall have occasion to refer to these views, and to adduce illustrations of their verity, in treating of the particular diseases of this class. I shall be well contented with the fruits of the few discourses which I purpose to devote to the subject, if they simply tend to convince you that there is no validity in the reasons usually assigned for neglecting cutaneous diseases.

Before proceeding to take up the individual affections embraced in the several classes, it may be useful to present a few general considerations applicable to the *pathology*, *management* and

diagnosis of diseases of the skin. I have already alluded to the fact that, in most of these diseases, (as well, indeed, as in a large proportion of affections of other structures), the true primary seat is not in the part affected—the skin—but the local morbid actions take place in consequence of some prior, internal or general disorder. To quote the expressive language of RAYER, the *roots* of cutaneous diseases generally lie within the system. They belong, in other words, among what it has been customary to call constitutional affections. This term *constitutional* it will be often convenient to employ. I will therefore briefly state what it is intended to denote. Not a few local affections, as they are classified and named in the nosology, in reality depend on morbid conditions other than those which can be seen or studied. They proceed from some general disorder within the economy, which science, as yet has not succeeded, to much extent, in explaining, or even in describing. In many, if not most cases, the fluids are probably at fault. Sometimes we know this to be the fact; but whether the humoral perversions are primary or secondary, is not, in all instances, an easy matter to determine. In our ignorance of the true character and seat of these general disorders, we must content ourselves, for the present, with merely designating them, collectively, by some name which will enable us to refer to the fact of their existence, as occasion may require. *Constitutional* is the title frequently employed for that purpose. We mean then, by a constitutional disease one that involves some inappreciable disturbance in the economy, occurring anterior to the local affection which presents itself to our notice—the nature of the former being frequently either obscure, or utterly unknown. Affections of the skin are, for the most part, constitutional in this sense of the term; and this is a very important fact to be considered, albeit the term conveys so little real information concerning their essential pathological character. In our therapeutical management, and in our efforts to obviate their causes (the latter often being essential to successful treatment), if we do not take into proper consideration their dependence on conditions of the system at large, we shall rarely meet with much success. The larger number of the most effective remedies in these diseases, are those which have some general influence within the economy, or, in

other words, remedies which are constitutional in their *modus operandi*. The following points, worthy of consideration in themselves, go to establish the view just presented:

1st. In the great majority of instances cutaneous affections are not referrible to direct, local external causes. This may be said to be true, as a general remark, of these diseases as a class. A few are apparently occasioned by the application of irritating substances, as when, for example, eczema attacks the hands of grocers or bakers. But it is by no means certain but that in instances like these the disease involves a constitutional predisposition, and the locality of the affection alone is determined by the external irritants.

2nd. There are various forms of cutaneous disease, and each form has its own peculiar characters. This in some measure is due to the different anatomical elements entering into the structure of the skin being affected repeatedly; but that ulterior causes are involved, is shown by the fact, that, to some extent, we can trace connections between special forms of disease, and certain conditions of the economy attributable to known causes. Thus a species of pustule called *acne rosacea* occurs usually in persons addicted to the use of spirits; certain forms of vesicular and pustular eruptions affect the face and head of children who are ill-fed and poorly cared for, and also during weaning and dentition.

Were we able to ascertain with precision in particular cases what the general perversions are, of which these cutaneous eruptions are the local expressions, we should understand why it is, that, under different circumstances, different forms of disease are manifested. But, with our present knowledge, we are here almost entirely in the dark. We can study effects, and sometimes trace to remote causes, but the intermediate links of the chain by which the causes and effects are joined, remain to be disclosed by future researches.

3rd. Diseases of the skin are apt to be obstinately persistent. They tend to protractedness in a large proportion of instances, showing that there exist internal, determining causes maintaining their continuance. The latter inference is shown to be rational by contrasting, as regards duration, spontaneous eruptions, as they are called by way of distinction, with those produced artificially

by some local irritant like croton oil, or the tartar emetic ointment. The latter usually speedily get well, after the operation of the local cause ceases. That the former do not, is doubtless attributable to the persistence of their internal or constitutional causes.

4th. Eruptive fevers are confessedly constitutional affections. Analogy leads to the conclusion that other eruptions are equally so, on the logical principle that resemblance in effects is presumptive evidence of a similar relation in causation.

5th. It has been already remarked that skin diseases are most frequently cured by remedies which exert some general influence on the economy—an influence called eutrophic, or alterative.

6th. Internal disorders are frequently relieved by the occurrence of eruptions; and, on the other hand, their repercussion either spontaneously, or by means of external medicaments, sometimes occasions internal disorders; showing that a morbific agent, or, at least, a morbific principle has undergone a change in location, or, as it is technically called, a *metastasis*.

7th. The appearance of cutaneous eruptions is frequently preceded by appreciable constitutional symptoms such as chills, febrile movement, disturbance of the digestive system, &c., which disappear, in some instances, so soon as the local affection becomes developed.

8th. Eruptions are produced by eating particular kinds of food, such as shell fish, and by some internal remedies as balsam copaibæ, iodide of potassum. In either case the agency in producing the local affection must be exerted by means of morbid conditions induced within the system.

9th. We have an instance in which various diseases of the skin are known to result from the introduction into the system of a special poison, viz: the syphilitic virus. Several specific forms of eruption proceed from this source, constituting a group which is sometimes distinguished as the syphilides. Syphilitic eruptions pertain to the subject of syphilis, and will therefore come within the province of my distinguished friend and colleague, the professor of surgery.

The foregoing considerations are adduced as establishing conclusively (in the absence of direct demonstrative evidence) the

constitutional origin of cutaneous affections in general. I have taken pains to present this summary of the more important of the facts from which this conclusion is logically drawn, because the doctrine is not alone interesting in a pathological point of view, but it is of great practical consequence, and is too often overlooked by practitioners in treating cutaneous diseases. It were much to be desired that science possessed more knowledge than is yet acquired of the essential pathological nature of this class of affections. For the most part, scientific researches have been hitherto limited, of necessity, to the description of their external characters. Dermatology is little more, as yet, than a branch of natural history. Specimens have been analyzed, compared and classified, and these are no mean results. It remains to ascertain upon what different internal conditions they depend. It may be long ere much useful information in this direction will be obtained, yet there is reason to hope they will ultimately be elucidated. I cannot but indulge the expectation that the appliction of analytical chemistry to the study of the fluids in health and disease, will, by and by, shed light on this interesting and important field of inquiry. Many considerations go to sustain the hypothesis that the proximate causes of most of the cutaneous diseases are humoral. It is not worth our while in this connection to discuss the merits of this hypothesis; since we should not be led by such a discussion, to take a decided position with respect to it. The prudent course is to preserve the mind sufficiently unbiased to be ready to receive facts and deductions as they are developed in the progress of science, and subject them to an unprejudiced examination.

The therapeutical course in general to be pursued in treating diseases of the skin, must be considerably influenced by the pathological views which have just been expressed. We are always to look to the constitution, obviating remote causes which affect it unfavorably, reforming habits, and improving its condition according to the indications which may be presented. We cannot always apply rational measures to the treatment of diseases of the skin, because we do not know the actual nature of the interior morbid perversions from which they spring. We can only follow what imperfect light we have. As regards the remedies to be employed,

it follows from the views which are here presented, and it will be seen hereafter to be the fact, that generally their supposed efficacy has been established empirically, that is to say, based on experience exclusively, not on any rational explanation which we are able to offer of the immediate effects which they produce.

ART. II.—A CASE OF SALIVARY CALCULUS.

BY H. K. PUSEY, M. D., OF GARNETTSVILLE, KY.

AN old black woman applied to me a few weeks since on account of a tumor on her jaw. The tumor was on the left side of the lower maxillæ, an inch below and half an inch in advance of the angle, and on examination was hard and did not appear to contain any fluid. The history given of the tumor by the patient was, that for the last thirty years she had, at times, been subject to swelling, pain, and tenderness about this part of her face, and that six weeks before she came to me the tumor began to form. It was then the size of a pullet's egg, tender to the touch, but without sensible fluctuation.

I directed the patient to apply a poultice and in a few days return and have the tumor punctured. The next day a spontaneous discharge of the contents took place, consisting of a small quantity of purulent matter and a smooth concretion an inch in length and three-eighths of an inch in diameter, of a pale-yellowish color, and weighing one hundred and forty grains. The body was fragile, pulverulent, and readily broke up into several pieces. It seems to be a *salivary calculus* formed in the duct of the parotid gland, which had been gradually increasing in size, and kept up irritation in the part for thirty years.

The growth of such bodies must necessarily be slow since the quantity of solid matter in the saliva is usually less than six parts in a thousand, of which only two-nineteenths are alkaline and earthy salts. The solid constituents of the saliva according to the analysis of FERICHS are ptyalin, mucus and epithelium, fatty matter, sulpho-cyanide of potassium, alkaline and earthy chlorides and phosophates, and oxide of iron. The composition of salivary calculi cannot vary materially from that of the saliva, and is indeed nearly identical with that of the "tartar" which collects on the teeth.

Concretions are not of so frequent occurrence in the salivary as in the lachrymal glands, but are nevertheless by no means uncommon. The most usual situation of these bodies is beneath the tongue, in the ducts of the sublingual glands, or in the duct of Wharton leading from the submaxillary gland, and the least common is the duct of Steno, of which only a few cases are on record. The case of which I have given the history above is to be added to the number. CRAIGIE mentions that these concretions are occasionally met with in the lower animals, and refers to instances in the mule and the ass, where calculi weighing six and thirteen drachms were found in the Stenonian duct.

It is only in rare instances that salivary concretions give rise to inflammation and ulceration, by which they are extricated, as happened in the case under my care. A case is recorded by SABATTIER in which the presence of a calculus in the duct of Wharton caused painful swelling of the submaxillary gland, thus giving rise to more suffering and inconvenience than the immediate swelling caused by the tumor. BOYER relates a case in which the submaxillary gland was inflamed and swelled, for which he performed an operation, extracting an oblong stony concretion, one extremity of which projected a little beyond the orifice of the duct of Wharton. An operation is generally necessary when these calculi become a source of annoyance.

ROKITANSKY, one of the latest writers on this subject and one of the most eminent pathological anatomists of the day, describes salivary calculi as "white, friable, and either round, oblong, cylindrical, or obovoid; in size varying from that of a millet-seed or pea, to even that of a hazelnut. They are either solitary, or,

if small, frequently very numerous (twenty and more); and they are composed of phosphate and carbonate of lime, held together by animal matter."

It is a curious fact that while healthy saliva contains so little phosphate of lime salivary calculi should occasionally consist of ninety-four per cent. of that salt. Dr. WALSHE conjectures that "the excess of phosphate is generated through the influence of irritation of the mucous membrane;" but I see no reason for resorting to such a supposition. In the first place, it is not easy to understand how the "mucous membrane" can "generate" phosphate of lime. Salts are not generated by the animal tissues, but merely separated by them from the circulating fluid. Earthy and alkaline salts are constant ingredients of the saliva, and it is natural that they should constitute the bulk of the salivary concretions being much less soluble than the other ingredients. There is no disease of the mucous membrane of the bladder to generate the calcareous matter of urinary calculi, but, a nucleus presenting, the phosphates and other constituents of the urine readily precipitated, collect upon it, and gradually form the concretion.

December, 1854.

ART. III.—TWO CASES OF STONE IN THE BLADDER. LITHOTOMY.

BY WILLIAM H. GARDNER, M. D., WOODSONVILLE, KY.

CASE 1. Samuel Shitton Clemmons, aged four and a half years. Oct. 22, 1853. Was a tolerably healthy child during his first year; at that age his mother had a long and severe spell of illness while he yet sucked. He became sickly and suffered much during his second summer with chronic diarrhœa, from which he recov-

ered slowly during the fall and winter. About this time he began to suffer pain in passing water, which has continued up to the present time, now two and a half years. On sounding him to day a calculus of considerable size was found.

Oct. 24. I proceeded to remove the stone, assisted by Drs. JETT, H. DAWSON, and Mr. HAMMON and Mr. BUMGARDNER, students of medicine, and Mr. W. L. KIDDER, chief engineer of Green River division of the Louisville and Nashville railroad. He was placed on the table at 12 o'clock; chloroform was administered by Dr. DAWSON, and the staff was given to Dr. JETT. I made the lateral operation in the usual manner, only I used the straight, probe-pointed bistoury in making the last incision in the neck of the bladder. The operation was completed without accident, difficulty, or delay, and the stone removed while the little patient was wholly unconscious. The stone was one inch and a quarter long, one inch wide by half an inch thick; composed chiefly of urate and phosphate of ammonia. During the evening he complained of smarting of the wound from urine passing; no hemorrhage.

25. Passed the night comfortably, only complained of some smarting of the wound. Directed him to keep quiet; no medicine whatever given, nor had he taken a grain by way of preparation before the operation.

26. Some fever; pulse accelerated, otherwise easy; urine by the natural channel.

27. Still doing well; urine all by the wound; dose of oil; bowels acted.

28. Patient doing well.

31. Patient still doing well; bowels rather active; opiate to check; urine all by the wound; no fever; appetite good; child cheerful and playful.

Nov. 6. Fourteenth day from the operation; wound healed; urine all by the natural course since day before yesterday; bowels have been rather active; opiate accordingly.

This patient was the son of a farmer who resided in Edmondson county, Ky., three miles east of the Mammoth Cave, on the elevated plateau or level of the country on which the Cave-House stands, which is, as estimated by Mr. KIDDER, about one hundred and fifty feet above the limestone. His father has never been

below the sand-stone formation and has used "freestone" water altogether. This stone is in some respects peculiar, and does not resemble any calculus in my collection of some twenty-six, all extracted from patients using limestone water.

CASE 2. Mary, a slave the property of the estate of the late Dr. LEWIS BARRET, of Munfordsville, Ky., aged about fifty years, born in Halifax county, Virginia; has resided in Kentucky the last thirty years; the mother of ten children, the youngest now thirteen years of age. She has usually enjoyed good health until the last few years. Some seven years since she began to experience difficulty in urinating; has been growing much worse for the last three years, most of which time she passed in the city of Louisville. Has voided several small calculi, four of which she gave me; the largest is half an inch in diameter, oval, flat, and very hard, it was passed off some twelve months ago. She now has frequent calls to pass water, attended with severe pain, straining and vesical tenesmus. Sounding detected at once the presence of a large stone.

June 3, 1854. Having derived little or no benefit from a few day's preparatory treatment, I determined to remove the stone as the patient's sufferings were very great. Assisted by Dr. JETT, Dr. BROWN, Dr. DAWSON, and Mr. BUMGARDNER, I proceeded to operate in the following manner:—I introduced a grooved director into the urethra and with the probe-pointed bistoury made an incision from below upwards, in the direction of the symphisis pubis, dividing the urethra in about half its extent. This admitted the forefinger at once into the bladder with which I dilated and searched the bladder, finding the stone, or rather stones, for there were two of them, large, I discovered that to remove them by dilating alone was out of the question. I therefore passed the bistoury, with the flat side on my finger, and incised both sides of the neck of the bladder, as practised by Mr. LISTON. This was sufficient to admit two fingers, but did not afford room enough yet to allow the stone to pass. I next divided the urethra downwards into the vagina extending the incision about two-thirds the extent of the urethra. The forceps were then introduced and the small stone extracted, after which the larger one was removed

with more difficulty. The two calculi weighed six ounces and a quarter. The patient was under the influence of chloroform administered by my friend, Dr. DAWSON, and although the operation was one of much severity she was wholly unconscious of what was going on. The bladder was examined and washed out with warm water, and the patient removed from the table, when she slept comfortably, perhaps an hour, after which she complained of smarting of the wound. An opiate was given.

4. Patient rested well last night; no fever or excitement.

5. Still doing well; patient altogether comfortable.

7. Patient doing well; appetite good; no action of the bowels since the operation; dose of oil to night; urine expelled involuntarily at intervals of two hours.

11. Patient doing well; appetite good; oil again to-day which acted well.

17. Patient wishes to be up, I do not allow her yet to rise; bowels acted last night without medicine, third action since the operation; she is able to retain the urine much longer; wound nearly healed.

24. Twenty-second day; wound all healed to small fissure at the orifice of the urethra; can retain the urine longer and rise to pass it off.

July 1. Patient well; up at work; retains her urine very well; when the bladder fills it becomes impatient to expel its contents, but gives her time to go aside.

December, 1854.

Bibliographical Notices.

ART. IV.—*Notes of M. Bernard's Lectures on the Blood, with an Appendix.* By WALTER F. ATLEE, M. D. Philadelphia: Lippincott, Grambo & Co. 1854.

This little volume has now been several months before the public, and much of the matter which it contains has been communicated to the profession from time to time by young American physicians resident in Paris. Some points, however, are entirely new, and the lectures of which it comprises the outline presents the most connected and complete view of the blood to be found in any recent work. We propose therefore to notice some of the leading topics of the work somewhat in detail, though conscious that in the months that have elapsed since its publication the subject has lost a portion of its novelty.

The blood is a product of the animal organism; it is fabricated by the living system, and is nearly the same in carnivorous animals and in those whose food is plants, but it is not identical in all parts of the same animal. The portal blood differs in its constitution from that of the hepatic vein. The effect of atmospheric air upon the color of this fluid is familiar to every one, but it is a mistake to suppose that the scarlet color imparted to it by oxygen is an indication of purity, since it has been shown by M. BERNARD that the same color may be induced by gases excessively noxious. The oxide of carbon, one of the most deadly gases known, renders the blood red to a very high degree, which is the more remarkable since carbonic acid, which in composition is so nearly identical with this gas, is known to give it the venous color. What is more remarkable is, that this change is persistent, the blood once rendered red by the oxide retaining its vermillion hue under every exposure. Prussic acid also reddens the blood, and further change

in color is prevented by its administration, the oxidizing property of the blood seeming to be at once arrested by the poison. A red color is, therefore, not always a sign of health. M. BERNARD once saw the blood flow so red from the vein of a typhoid patient that he supposed it, at first, to be arterial. The change from arterial to venous does not take place in the blood when nervous influence is suspended.

The temperature of the blood has generally been stated to be higher in the left than in the right ventricle, but M. BERNARD asserts that the reverse is the case, the difference amounting to about one degree Fahr., in favor of the right side of the heart. The maximum of temperature is in the portal vein. As the blood passes through the lungs it is cooled by the air. If the air "does so in the fingers why," asks M. BERNARD, "should it not do so in the lungs?"

The temperature of the blood cannot be much raised without fatal results. MAGENDIE showed by decisive experiments that animals in a heated atmosphere seemed to suffer very little, at first, but then died suddenly, and that their blood was hotter by from twelve to fifteen degrees Fahr. A higher temperature seems to be incompatible with life.

The quantity of blood varies somewhat in animals relatively to the weight of their bodies. In the dog it is one to four and a half; in the sheep one to five; in the cat one to five and a half; and in the rabbit one to six and a quarter. In a man weighing one hundred and forty-five pounds, M. BERNARD estimates the quantity of blood at thirty-two pounds. The quantity augments at each moment of digestion, as is proved by the rising of the mercury in a barometer attached to an artery; at the same time the liver and spleen swell, and are only made to diminish by secretion from the lungs and the kidneys. "In fasting an animal," says M. BERNARD, "consumes his blood, and dies just as in hemorrhage."

The blood flows with greater facility through the capillaries than a less viscid fluid would do. M. BERNARD injected water into the kidney of an animal by the renal artery, and found the tissue of the organ in a short time infiltrated and the circulation finally arrested. If in place of water serum of blood was injected

no infiltration occurred. This fact affords an explanation of some interesting morbid phenomena. It is well known that the loss of blood will give rise to all the symptoms of plethora or capillary congestion, for which practitioners have in times past committed the serious mistake of resorting to blood letting. The headache, throbbing of the temples, intolerance of light, and other allied evidences of inflammation or congestion of the brain are the natural consequences of copious hemorrhages, and are relieved, not by depletion, but by remedies which increase the viscidity of the blood. "In order that the globules can circulate, the existence of fibrine is necessary. When you inject serum containing globules, or when the blood is defibrinized and the part is examined under a microscope, the globules are seen to be all at the bottom of the vessels, and at last the small vascular ramifications are blocked. The fibrine of the blood, then, keeps the globules from falling to the lowest part of the vessels; lighter than they are, it surrounds, envelopes, and separates them."

The rapidity of the circulation is affected by acids and alkalies, the latter increasing and the former retarding it, and other agents are also found to exercise an influence over the flow of the blood, as nitrate of potash and alcohol. The bearing of these facts upon pathology will be readily perceived.

Albumen, one of the most important constituents of the blood, modifies materially its solvent power, enabling it to dissolve many substances which naturally are not soluble in an alkaline liquid. It is the albumen which holds in solution the metallic matters in the blood, most of which, after a time, are deposited in some organ, as the albumen is appropriated. Thus antimony, lead, and arsenic find their way to the muscles, liver, and other organs.

The blood is the theatre of incessant changes. The phenomenon of calalysis takes place in it with the greatest facility, and it may be loaded with carbonic acid by injecting yeast and sugar into the vein of an animal, or rendered poisonous by the introduction at the same time of emulsine and amygdaline into the circulation.

It has lately been ascertained that the blood undergoes crystallization as well as coagulation when withdrawn from the body. This curious fact was discovered by OTTO FUNKE, and the blood

employed by him being that of the spleen it was supposed to be peculiar to the blood of that viscus, but the experiments being multiplied it was seen that the vital fluid drawn from any part might be made to crystallize. It takes place more easily when a drop of water is added to the blood. When the crystals are made to form as large as possible, they are found on analysis to contain a large quantity of iron.

The fibrine of the blood, it would seem, may be increased by repeated blood-lettings, but it is not the same as the normal element; the fibres are shorter; it is less elastic, and resembles fibrine only in separating the globules from the rest of the blood. Macerated for twenty-four hours in warm water it is dissolved and presents the characters of an albuminous solution, giving support to the idea that fibrine is only transformed albumen.

The relation of the kidneys to the blood is one of great interest to the pathologist. These organs are prominent among the depurators of the economy. BERNARD found that when substances generally elsminated by the kidneys were injected into the blood, they made their way into the stomach, and comparative anatomists have discovered that in the inferior animals, where there is no kidney, it is through this viscus that the eliminution takes place. The vomiting and derangement of digestion in persons affected with disease of the kidneys, it is probable, have their origin in the disturbing effect of urea in the alimentary canal.

The presence of sugar in the liver as a normal secretion, is a discovery for which physiological science is indebted to M. BERNARD. BUCHARDAT had found it in the blood of diabetic patients, but it was believed to be there as the result of disease. MAGENDIE subsequently showed that it was always present in the blood, as a normal constituent, but he supposed that it came from sugar introduced from the exterior. BERNARD has proved by a series of observations that it does not come from the aliment, but is generated by the liver whatever the character of the food used. It is not detected in the clot, and is destined to be eliminated, not as sugar, but as carbonic acid, the lungs destroying what the liver produces. The lower vena cava carries the sugar from the liver to the lungs through the right side of the heart, where it undergoes combustion. When but little sugar is made by the liver all

is destroyed in traversing the lungs, but when more is formed there is an intermediate state, namely lactic acid, and when the quantity is more than one part to a hundred of blood, it may pass off by the kidneys and the patient become for the moment diabetic. At the commencement of diabetes, according to M. BERNARD, and also when recovery is taking place, sugar is only found during digestion.

It is from the iron contained in the blood, M. BERNARD holds, that the hematine possesses the property of absorbing oxygen, and it is only in the hematine that the iron is capable of fixing itself. Should there be no hematine, therefore, the simple administration of iron in anemic diseases would not be sufficient; the want of iron may depend upon the deficiency of hematine. Iron differs from the other metals occasionally detected in the blood in these circumstances, that it is only found in the globules, and never appears in the secretions, whilst, lead, copper, arsenic and the others localize themselves in the liver or muscles, or pass off with the secretions. No other metal but iron has been shown to be capable of playing a physiological part, though there is reason to believe that manganese may.

The effect of vascular plethora upon certain secretions has long been observed by practitioners. M. BERNARD shows by experiments upon dogs that the renal secretion is vastly increased by tying the arteries of all their limbs, by which the pressure upon the vascular tissue of the kidneys was raised from seventy-six to one hundred and twelve millimetres. The quantity of urine declines under venesection and is augmented by the injection of blood. In many cases, therefore, the excessive secretion of urine depends upon mechanical causes, but not in all; in some nervous affections there is an exaggerated amount of urine without any undue pressure upon the circulation. Pressure exerts no influence upon the salivary secretion, and the same is true of the function of many other glands.

"If these two secretions, the salivary and the urinary, be reflected upon, it will be seen that they could not act under similar influences, that they could not be regulated in the same way. The secretion of urine is constant, and must be under the influence of pressure, for it is connected with the purification of the blood. The salivary secretion is connected with certain acts of the diges-

tion. The excretions must be distinguished from the secretions, which are not thrown out, whose active principles do not previously exist in the blood, but are formed in the organs, and are connected with certain acts of the economy. All such organs having these active principles in themselves, are not influenced by the external pressure, while those acting the part of a filter always are.

"The liver is probably between the two, between secretory and excretory. Suppose an obstacle, a calculus, for instance, to oppose itself to the excretion of the urine, the arterial pressure being resisted, the kidney will no longer secrete; the products usually excreted accumulate in the blood, and the accidents resulting from their non-elimination take place. When there is an obstacle to the elimination of the bile, those principles that certainly previously existed in the blood, are then retained, and the individual is jaundiced. But nothing similar can ever take place in the salivary glands; they become swollen, the products remain there, and never are found in the blood. Independently of these organs, there are many others that perform their functions differently as the pressure varies. As a general rule, the functions, cerebral, muscular, &c., diminish with the pressure. The action of coffee has thus been explained by some physiologists."

We have already seen that life is bounded by a limited range of temperature. A certain degree of heat is essential to all vital processes. Digestion, secretion, the metamorphosis of matters in the blood only go on when the vital heat is normal. The presence of oxygen favors these actions. Sugar disappears from the blood at the moment of the change to arterial, probably on account of the superabundance of oxygen. The changes that take place in the blood M. BERNARD does not hesitate to refer to *fermentation*. He says:—

"Arterial blood absorbs oxygen and abandons it, and this absorption varies and disappears, under the same circumstances as many fermentations, as, for instance, upon the lowering of the temperature. In making beer this is seen, and again we see that it ceases when the heat is raised above seventy-five degrees Centigrade, by the destruction of the organic principle producing the phenomenon. So in the blood, oxygen is absorbed in smaller quantities in the cold, and the effect is the same whether the blood be cooled artificially in a vase, or in the body. By raising the temperature the oxygen is found to be absorbed in proportion as the temperature rises. At forty-five degrees Centigrade, however, this principle in the blood, that absorbs oxygen is destroyed. Magendie has made many experiments on keeping animals in

high or low temperatures; after long resisting the influence of heat, they finished at the end of a certain time by becoming warmer, but they never became hotter than forty-five degrees, when they died. It is precisely at this temperature that the blood ceases to absorb oxygen. This property is also lost when the temperature is very low. In the one case the blood is everywhere very red, both in the veins and the arteries; in the other, after the elevated temperature, on the contrary, it is everywhere black.

"There are then in the blood organic matters whose existence there is necessary for the absorption of oxygen. The phenomena which take place under their influence are the same as those of fermentation. All the liquids that act in digestion, act in this way; the gastric juice acts in virtue of an organic principle, and the pancreatic juice also. Pepsin has been found; it is purely and simply a ferment; experimenting with it, digestion will be found to commence at 10°, to increase to 40°, and then to diminish to 60°, when it ceases altogether. Without knowing this, Spallanzani made experiments on snakes, leaving them in the cold, and he found them to remain fifteen days without digesting. It is the same thing with the pancreatic juice, which ceases to act when the heat is so great as to coagulate it. This ferment acts in an alkaline medium, pepsin requires an acid one; that of the blood must be alkaline. Neutralize the gastric juice, and it no longer digests; it may be said that this is because it is the acid that digests, but take it alone and it does not. It is the *ferment* that digests, it must not be supposed to be the acid. The blood to absorb oxygen must be alkaline; it is normally so, but it is not the alkali that gives it this property, for a temperature of forty-five degrees does not destroy the alkalinity; it ceases then, because at that point the organic principle is destroyed. This special ferment has not as yet been isolated in the blood, but if none existed there, temperature would not exert the influence it does, upon its properties.

"Arterial blood never presents a buffy coat, as the venous is well known to do. This is easily observed in animals after death, when the white clot found in the right ventricle is never seen in the left. This might put us in the way of finding a reason for this buffy coat; it would appear as if, in traversing the lungs, a more intimate connection took place between the lymph and the blood."

The blood of the portal veins differs in many respects from that of other parts of the body,

"It is darker than other blood, the difference being very great when compared with arterial; though not so marked when compared with that from the hepatic veins, it was still very evident. It coagulates more slowly than any other blood, and the clot is

diffluent, soft, scarcely solidified; it never presents a buffy coat, though it coagulates so slowly, a circumstance favorable to its formation. It absorbs a very great quantity of oxygen. In some experiments made the year before, M. Bernard observed that in this respect all bloods do not resemble each other. That of the vena portarum absorbed the most, from 20 to 25 per cent., that of the right ventricle, 18 to 20, of the jugular vein, 14 to 15, while the arterial blood only took up from 5 to 6 per cent. This property varies in different animals, the warm-blooded absorbing much more than the cold. Temperature also varies this absorbing power. This is enough, however, on this point for the present.

"The portal blood has to a very slight degree the property of becoming arterial in color. If venous blood be shaken in oxygen it becomes red. It is supposed that when oxygen is absorbed, that the blood is red, and when given up that it is black, and this is generally true, though not always so; the vein of the kidneys carries red blood. The clot of the blood from the vena portarum never becomes red on its surface; that of the hepatic veins does: the portal blood is the only one that never does become red on exposure to the air. This blood contains more iron than any other, and the iron may affect the amount of oxygen absorbed, but cannot affect the color. This fact seems to show that not only the iron must exist, in order that oxygen be absorbed, but also that it must be combined with something in order that the blood become red. There is more iron when the blood enters the liver than when it comes out; there is some elimination of iron in the bile. In the liver the blood loses iron, and there is also a diminution in its property of absorbing oxygen.

"As the portal blood possesses, then, properties very different from the arterial, it must have undergone a change in passing through the intestines. During digestion, other phenomena are seen; the blood in the vena portarum is much more abundant, and circulates much more freely. It contains more water, and many substances are found to have been acquired. Saccharine matters can be there under several forms, as cane-sugar, grape-sugar, and lactic acid. Fecula can never be absorbed as such; it must be previously changed; it must be rendered soluble, so as to be digestible; it changes to grape-sugar. Cane-sugar is partly changed to grape, and partly absorbed as it is.

"The portal is much less black during digestion than it was seen to be during abstinence, and its property of absorbing oxygen is diminished. Sugar has been shown by M. Bernard to diminish the property of the blood of absorbing oxygen.

"In regard to the albuminous bodies, the direct proof of their existing in greater quantity in the portal vein during digestion than during abstinence, is perfectly impossible. With sugar it is

very easy, but with these it is different, as albumen and fibrine are always existing there. M. Bernard, however, made the following experiments that are very conclusive on this point. When he placed in the intestine, albumen, either of the egg or of serum, it disappeared, and none was found in the excrements. When, instead of that, he put some into a vein, he always found it in the urine; so that here it played the part of a foreign body, and was thrown off as such. What difference is there in the two conditions? In the one, being placed in the jugular vein, it went at once to the heart, but in the other it went previously through the liver, and it is here that the necessary modification takes place. That it is not from any action exerted by the intestines, is shown by this, that upon throwing it directly into the vena portarum, none made its appearance in the urine. None is ever found in the urine when it is forced to pass through the liver before entering the arterial circulation. The same thing is true of cane-sugar, which is always found again in the urine, after being thrown into the jugular vein, yet none is ever found there if it be thrown into the portal vein. Now, if the chyliferous vessels absorbed albumen, they would empty it into the subclavian vein, it would go into the arterial system before passing through the liver, and would be eliminated as a foreign body. It must hence be judged that albumen is not absorbed by the lymphatics, but that it enters together with the sugar into the portal vein.

"While the other *bloods* then remain about the same at all times, the blood in the vena portarum varies very much with what is eaten, and *above all* with the time. The blood in the arteries of herbivorous and of carnivorous animals is not essentially different; that in the vena portarum varies with the alimentation and with the period of digestion."

In regard to the source of sugar in the liver M. BERNARD says:—

"We can say then that the sugar is formed at the expense of the azotized or saccharine matters. The effect we have just seen fatty matters to have, and the special apparatus which has been demonstrated for their absorption, all go to show that they do not pass through the liver. The ciphers for the quantity of sugar formed when gelatin and fecula are given, are about the same as the ciphers representing the normal quantity in the blood.

"This sugar is chemically the same as glucose, but undergoes transformation with much more readiness. As the source of this sugar, fecula could only be called upon in animals having it in their food; carnivorous animals eating nothing capable of undergoing the transformation, nitrogenized matters must in them supply the material. In them, there is a diminution in the quantity of azotized matters in the blood, after traversing the liver, but a new principle, sugar, is found. As azote is not found in the

sugar, it must have been separated in the liver, and it is found in the bile, by which means it is eliminated.

"This sugar is intimately connected with the functions of life. The quantity is normally the same in all animals, there being about 1-50 per cent. in the hepatic blood of both the carnivorous and the herbivorous. The greatest quantity M. Bernard has ever found was two per cent., and that was in a monkey who had eaten meat, butter, and carrots.

"This quantity of sugar must be mixed with all the blood of the body; already in the right ventricle, the proportion is much less, being only 0·69 per cent., as will be seen hereafter, in following the blood beyond the liver.

"In vertebrated animals, the sugar, being immediately emptied into the vena cava, is spread at once over the body; in the invertebrated, in place of going away by the hepatic veins, it goes back to the intestines by the biliary duct, and is absorbed there by a special apparatus before going to the respiratory organs.

"The quantity of sugar coming from the exterior, is very slight. When no saccharine matters are eaten, none is ever found in the portal blood, and when they are, you find only some traces of it; they may be taken up as lactic acid, &c. In the hepatic veins you have during digestion 1·50 per cent., and during abstinence one per cent., in *fresh* blood. The proportions given by Regnault are much more feeble; probably he was not aware of the great destructibility of this sugar. If it be not examined at once, a very great difference will be found; at the end of a few hours, in summer, when the destruction marches very rapidly, there is scarcely any. The sugar of diabetes is exactly the same as that of the liver, and it is also much more destructible than the others, being about twice as much so as ordinary glucose. The greater destructibility of the animal glucose, so to speak, over the vegetable, from which it cannot be distinguished by chemical tests, is shown by the following experiment. Under the skin of two rabbits, whose urine, previously tested, showed no trace of sugar, he injected, by means of the canula of a trocar, solutions of these two sugars. The experiment might be performed by injecting them into the veins, but in this case it might be objected to the result that too much had been suddenly introduced into the circulation, and on that account some would be found in the urine. When they are introduced under the skin, and thus made to undergo absorption before entering the circulation, this objection cannot be made. In the case in which the animal glucose was injected, no sugar was found in the urine when it was tested by the liquor of Barreswil, in the urine of the other rabbit it was. As said before, one is about twice as destructible as the other.

"The variations in the quantity of sugar have no relation whatever to those in the quantity of bile. Several modifications of the

blood of the vena portarum occur in the liver, and they are independent of each other. It must not be thought that digestion, that is to say absorption, for absorption is the true digestion, commences immediately after deglutition. This absorption takes place several hours after eating. Five hours, then, after eating, when digestion is in full activity, we have the greatest quantity of sugar, and at this time, the flow of bile is feeble. It is when about ten hours have elapsed since eating that the flow of bile is greatest. This is perfectly conformable to what is seen in invertebrated animals, in whom, as has been said, the sugar returns by the biliary duct to the intestines. In them, at the time of digestion, you see a colorless liquor flowing by the duct; when digestion is finished you see a colored; the first is sugar, the second is bile."

We have read these Notes of the lectures of M. BERNARD with deep interest and are sure that the profession will thank Dr. ATLEE for the opportunity of becoming acquainted with the peculiar views of one of the first physiologists of the day.

ART. V.—*A Systematic Treatise, Historical, Etiological, and Practical, of the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux varities of its population.* By DANIEL DRAKE, M. D. Edited by S. HANBURY SMITH, M. D., Professor of the Theory and Practice of Medicine in Starling Medical College, Ohio, and FRANCIS G. SMITH, M. D., Professor of the Institutes of Medicine in the Medical Department of Pennsylvania College, Philadelphia. Second Series. Philadelphia. Lippincott, Grambo & Co. 1854.

The gifted author of this work, as is known to all our readers, was cut off in the midst of his labors. His first volume had been published and he had advanced far in the completion of the second when he was called to resign his task into the hands of others. The editors of his papers have executed the trust committed to them faithfully and industriously. We regret that a more popu-

lar house was not chosen for its publication, but this ought not to prejudice the sale of a work intrinsically so valuable.

This treatise was the great labor of Dr. DRAKE's industrious life. He had meditated a work on the diseases of the great central valley of America for more than thirty years. Ten years before he died he had collected a great mass of materials for such a work, and for a number of years he traveled extensively to acquire a personal knowledge of the medical topography of the country and of the diseases which he was about to describe. His success was commensurate with his immense toil. On all hands, his work, when it appeared, was pronounced the most creditable to our medical literature that any American pen had produced.

The second series will be hailed with uncommon pleasure by a large class of readers. It goes far towards completing the plan of its lamented author, and forms a volume which ought to find a place in the library of every American physician. The first volume consisted mainly of an account of the topography and climate of the country, concluding with a very thorough description of autumnal fevers. The editors of the second volume have wisely concluded to republish these concluding chapters, giving in one volume all that Dr. DRAKE had written on the subject of fevers.

An elaborate discussion of yellow fever occupies nearly two hundred of these closely printed pages, which will be consulted by every reader who desires full information on the subject. Dr. DRAKE himself never saw a case of yellow fever, yet, as he says, he was "not precisely in the condition of a historian, who might have only printed works from which to compile," for he visited and studied "the topography, hydrography, and climate of a number of the most infested localities," and, what he regarded as of equal or greater advantage, saw and conversed with a number of physicians, who had become familiar with the disease in all its aspects. Under the head of geography and chronology of the disease, he gives the number of times that it has prevailed sporadically or as an epidemic in the Valley of the Mississippi river and its borders, and from his table it appears that up to 1849 no year had passed since 1817 without more or less of the fever. The history of the fever in this region reaches back as far as 1702. We cannot follow the author into details respecting the local his-

tory of the disease, which however possess great interest and will reward the careful study of the etiologist. After presenting a long array of facts on this subject, he makes his etiological deductions which we copy in extenso.

"Let us now proceed to ascertain the bearings of the facts which have been set forth, on the origin and spread of yellow fever.

"Passing by the vexed and difficult inquiry, whether the yellow fever of Havana originated there, or was introduced from abroad, we may speak and treat of it as a disease of that city; seeing that although it is not extensively prevalent every summer, it is never absent, and thus, if not a native, is a naturalized endemic. In reference to the first case that ever occurred in that city, it may be said that if the cause was generated there, the subsequent prevalence of the disease has no doubt depended on the continual generation of the same cause.

"If the disease was imported, we may assume, with equal confidence, that the introduction and continuance of the fever have depended on one of two causes,—either a contagions emanation from the bodies of the sick, in perpetual succession, or a peculiar fermentative power in the portion of empoisoned air originally imported, whereby it transformed that of Havana into its like, and has ever since kept up the disease.

"But the same views are not applicable to New Orleans, where a colder winter might destroy the cause of an imported disease, which would not re-appear till it was re-introduced. Hence the fever may prevail annually in Havana, although originally an exotic, but be frequently extinguished in New Orleans, and require to be imported anew.

"Our first inquiry relates to contagion. In the prosecution of this, the rules of logic require the exclusion of all cases in which the disease is said to have been contracted by going on board of a ship or boat where there were yellow fever patients, or by visiting a patient in a town affected with the fever when it is epidemic, for the individual in these cases is exposed to the same atmosphere which may have produced the fever in the sick, and it cannot be told whether he contracted the fever from them, or from entering the same localities which had occasioned it *in* them. We must also, for the same reason, exclude the cited instances of epidemic fever soon after the arrival of a ship or boat, because it may have brought a portion of the atmosphere from the sickly port whence it sailed, or may have arrived at the moment when the disease was about to appear from local causes. Thus restricted, the number of facts, going to show the propagation of yellow fever by contagion is surprisingly small, and most of them far from being unquestionable.

"1. When speaking of the introduction of the yellow fever into Opelousas, in 1837 and 1842, we gave an account from Prof. Carpenter's book of the apparent successive propagation of the disease from two strangers who arrived there from New Orleans and sickened of the fever. It will be recollected that steamboats do not reach this town. But it must be remembered that in the former of these years the disease was extensively prevalent; in the latter, however, it was limited to New Orleans and Mobile.

"2. When the body of Dr. Smith, of Plaquemines, was taken to New Iberia, it was said there was no fever in the village, but that nearly all who visited the church in which the corpse was deposited or attended the funeral, were soon afterwards seized with the fever. This was in the year 1839, when the disease was more extensively prevalent than in any other year.

"3. Under the head of St. Francisville there are details which show an apparent propagation of the fever from one person to another; but the fever that year was extensively epidemic, and previously to the occurrence of the second link in that chain of cases, it was prevailing in the adjoining village of Bayou Sara.

"4. When treating of the epidemic of Woodville, we found that on a certain plantation, a number of negroes who had not visited the town experienced attacks of the fever after the death of one who had contracted it there; but the author of this important observation likewise informs us, that the disease occurred on some plantations, where, as far as he could ascertain, it had not been introduced. There were, indeed, signs of an epidemic constitution for several miles around Woodville.

"5. By referring to the history of the yellow fever in New Orleans, for the year 1841, the reader will find an account from Dr. Beugnot and Luzenburg, of its apparent introduction and spread by officers and seamen of the Talma, anchored off La Fayette, adjoining the city, where, as they state, the fever had not in any previous season commenced.

"If the history of the disease for the last fifty years in the Valley of the Mississippi furnishes any other facts going to show the contagiousness of yellow fever, I have not been able to find them. There are, however, two analogical arguments in support of this opinion, which must not be overlooked.

"1. The fever is not intermittent, but approaches to the continued type. Now typhus and typhoid, which are continued fevers, are known (or believed) to generate contagion; and from analogy we might predicate the same of yellow fever.

"2. We have seen as a general fact that yellow fever affects persons but once, which, as all the world knows, is true of the eruptive fevers, and as they are contagious, we might conclude that yellow fever is the same. But on this method of analogical reasoning, yellow fever ought to be eruptive, as well as contagious.

If it fail in one it may fail in the other also. The relation between eruption and contagion is in fact much closer than between immunity from second attacks and contagion, for typhus fever and syphilis are contagious and eruptive, yet second attacks of the former are not uncommon, and it is not proved that an attack of the latter bestows any immunity against future infection.

"There is, moreover, a difference between yellow fever and the true eruptive fevers, as to exemption from second attacks; for residence in a cold climate destroys it in yellow fever, but no change of climate has that effect in the eruptive fevers. Thus the contagiousness of yellow fever cannot be established by this any more than by the other.

"Let us now examine the facts which oppose the doctrine of contagion.

"If we look to small-pox, measles, and scarlatina, the best examples of contagious fevers, or even add to them typhoid and typhus fevers, we find that they propagate themselves in all seasons, localities, and conditions, provided the emanations from the body of the patient be not decomposed or wafted away; but such is not the case with yellow fever.

"1. Thus, vessels arrive in New Orleans in the months of April, may, and June, from the Havana, when yellow fever is prevailing there, but the cases of fever which they bring, do not propagate the disease. Yet if contagion exists, why is it not as operative in those months as in August and September? It certainly differs from all other contagions, if it cannot exert itself as well *before* as *after* the summer solstice.

"2. As soon in autumn as the cold becomes great enough to freeze the ground, yellow fever ceases, but cold does not, any more than heat, put a stop to small-pox, measles, and typhoid fever. And here again yellow fever fails to stand the test.

"3. The fevers just mentioned prevail both in town and country; but yellow fever is almost limited to the former;—another failure.

"4. Small-pox, measles, and scarlatina, are propagated and become epidemic in salubrious, as readily and certainly as insalubrious localities; which is not true of yellow fever.

"Thus we see that this disease does not conform to the laws of contagious fever. But stronger facts remain to be stated.

"Nothing connected with the whole subject is more definitively settled than that persons may die of yellow fever, both in town and country, without communicating the disease. Many accurately-observed and well-attested cases of this kind are scattered through our historical narratives; and a much greater number might have been given. Now, such events in such places are the true tests of the contagiousness of this disease, for no other alleged cause is present. In New Orleans and other commercial cities,

in the latter part of summer and early autumn, there is another cause; and how is it possible to analyze the phenomena, and say that one depends on that cause, and another on contagion? It may be said, however, that the city atmosphere is not the direct cause of any of the phenomena, but the cause of the contagiousness of the fever. Thus, if two individuals in the same ship had received the *semina* of the fever in Havana, in the month of August, and one should be landed on a plantation below the city, and the other brought into it, and both should sicken, the city atmosphere would so act on the system of one as to cause it to secrete contagion, while the system of the other, surrounded by a pure air, would not secrete contagion—one would communicate the disease, the other not. As I know of no fact or analogy going to support this hypothesis, I am compelled to reject it; but must not do so without remarking that it recognizes the necessary, though indirect, agency of a particular local condition of the atmosphere in the production of the fever.

"But another view may be taken of this matter, which is, that the *predisposing* impress of impure air is a necessary condition to the action of the contagion, and consequently, that it produces no effect beyond the limits of such an atmosphere, and therefore, *seems* not to exist, when it really does. According to the preceding view, contagion is not produced without the influence, *on the body of the sick*, of an impure atmosphere. According to this view, contagion is inert with the predisposing influence of an impure atmosphere *on the bodies of the healthy*. This recognizes the action of two causes, and if either be absent, the fever will not appear. It may not be called the hypothesis of compromise, for it tolerates, or rather requires, all that both parties demand. As we have many examples of co-operative and successive influence of remote causes in the production of disease, this hypothesis wears a most plausible aspect, and has drawn to itself nearly all who once held to the doctrine of the sufficiency of contagion, and not a few of those who formerly believed in local causes exclusively. It accounts, in a general way, for the absence of the fever from filthy, but insulated towns, where but one of the causes exists; for its frequent prevalence in commencial towns, which hold intercourse with those further south; for its not appearing in towns which are free from impurities, although cases of yellow fever may be introduced into them; for its prevalence in autumn, when the air from the heat of summer is most impure, and for its cessation in winter, when the further development of exhalations is arrested by frost. It inculcates cleanliness to prevent the development of the predisposing cauee, and non-intercourse with places where the fever is prevailing, to exclude the exciting cause; prudently attending to both, lest the efforts to suppress either should be unsuccessful.

"Now the objections to this hypothesis are:—

"1. That it combines an admitted and an assumed cause; the local contomination is a fact—the contagion an assumption.

"2. That in many invasions of the fever cases have occurred under circumstances which almost preclude the possibility of communication with the sick, and must therefore be referred to the admitted local cause.

"3. That the fever often appears almost simultaneously in various parts of a town, in the same manner as epidemic cholera, or as autumnal fever among the inhabitants of the country.

"4. That it has (as we shall hereafter see) commenced and prevailed in ships, and in some localities on land where the alleged exciting cause, contagion, could not possibly be.

"5. That having admitted one cause, which appears sometimes to have been sufficient, of itself, for the production of the disease, it is unphilosophical to admit another, the existense of which has not been proved.

"There is however anotner view which avoids the last objection, while it comprehends both local contamination and contagion. According to this hypothesis, contagion acts upon the impurities suspended in the atmosphere of places where and when the fever prevails, and excites in them an intestine or fermentative action, which transforms them into its own nature. This hypothesis of contagious fermentation proceeds on three assumptions: first, that contagion is a ferment; second, that the gas or exhalation, which contaminates the atmosphere, is susceptible of fermentation; third, that the produch is the same as the ferment which excited it. If either of the former of these assumptions fail, the hypothesis likewise fails, for there will be no fermentation. But not so with the latter; for it may be that the exhalations from the bodies of yellow fever patients never produced the disease in others by their direct action, are not in fact contagious, but only ferments, capable of transforming the impurities of the atmosphere into the true and only cause of the fever.

"The hypothesis of fermentation certainly avoids some of the objections which lie against that of contagious propagation by personal intercourse, as we may admit, that it can go on rapidly and in a short time transform the ærial impurities of a city into a poison, like itself, so that the fever may be produced simultaneously in various parts of the locality; it also explains the production of a fever atmosphere in a particular portion of a city; and finally accounts for the production of the disease in those who only ride through that part, which the doctrine of personal contagion does not.

"Nevertheless, it must not be forgotten that this hypothesis rests entirely on assumptions, as indeed the whole doctrine of gaseous or ærial fermentaiion does, and is, moreover, liable to some objec-

tions which I shall proceed to state. But previous to doing this, it will be proper to bring forward and incorporate with this, another branch of our subject. In the beginning of our inquiry, it was stated hypothetically, that if yellow fever were originally introduced into Havana, and thence upon this continent, it might have been done either by the importation of contagion, or of a portion of that atmosphere, or rather of its like, which produced the first case, and which acted as a ferment in the air of Havana. Now what I am about to say, will apply equally to both these alleged ferments, and embrace every possible case of importation. When a ship arrives in New Orleans from a yellow fever port, and brings patients with that disease, she has, we may admit, within herself, all the causes which prevail in the port from whence she sailed, the local summer atmosphere and the emanations which the sick send forth; and those who visit her may contract the disease precisely as if they had visited the town from whence she sailed; but to make her arrival the cause of a subsequent epidemic, it is assumed that her foul atmosphere is the immediate cause of a transformation of the impurities suspended in the atmosphere of the city into the same kind of poison with itself, and thus she raises an epidemic. She does not merely add her own poisonous air to the impurities already existing in the locality, but impregnates them with a fatal leaven.

"Now can these alleged ferments act on every kind of gaseous impurity? Certainly not. For when introduced into the atmosphere which produces autumnal fever, they excite no fermentation, effect no transformation, occasion no yellow fever beyond their own limits. When yellow fever is epidemic in Mobile, for instance, steamboats run daily up the Alabama river, and lie at the wharves of Kahawba, Selma and Montgomery, while their inhabitants are suffering under remittent and intermittent fever, yet no yellow fever appears. At New Orleans, while yellow fever is raging in the city, and especially near the river, the environs which lie contiguous to the cypress swamp in its rear, are ravaged by remittent and intermittent fevers. Lastly, up the Mississippi river, it is chiefly in the older towns, where the malarial impregnation has become feeble, and autumnal fevers are comparatively rare, that yellow fever makes its appearance. If this were not the fact, the leaven of yellow fever would transform the malaria of autumn into the cause of that disease, and spread it over the whole country. We may take it then as a settled fact that the yellow fever leaven exerts its transforming power only on the impure air of certain localities, and that in the absence of such impurities, there is and can be no spread of that disease. We have then in this state of the atmosphere over yellow fever localities a *conditio sine quâ non* to the spread of that disease from the vessels which import the alleged ferments. But is this malaria of our commer-

cial towns and cities inert and harmless of itself? By no means. For when and where yellow fever does *not* occur, these localities in summer and autumn are infested with a remittent fever, which bears so striking a resemblance to the remittent fever of the country, that every physician regards them as the offspring of the same cause in a state of modification. In the country this agent is generated in swamps, and other humid places, where the recrements of plants and insects are subjected to decomposition, in the towns where yellow fever oftenest prevails, other vegetable recrements, animal exuvia, and exhalations and secretions from a dense population, are subjected to the same decomposition. As the decomposable materials are not identical, the products of fermentation cannot be identical. The rural malaria produces one modification of fever, the civic another. Now is it not remarkable, when these malaria approach so near to perfect identity, that the imported ferment should readily change the civic malaria into yellow fever gas, but produce no effect whatever on the rural malaria? When the two, judging by their effects, so far from being distinct species are not even striking varieties, how is it that the foul air of Havana or the exhalations from the body of a yellow fever patient can start one into active and deleterious fermentation and be utterly inert when mingled with the other? The hypothesis of ferments is bound to answer this question; or else to establish itself by positive facts.

"This latter it has attempted to do in the following manner:—

"1. Ships from Vera Cruz or the Havana have almost always arrived in New Orleans just before an outbreak of the fever.

"2. During the war with England, when the intercourse with the more southern ports of the Gulf was suspended, or nearly so, the fever was absent.

"3. Those places of the Gulf which have most intercourse with Havana, are oftenest invaded

"4. The disease has almost confined itself in the interior to the towns which are visited by steamboats, and has occurred oftenest in those which are oftenest visited.

"5. They have frequently been invaded immediately after the arrival of a boat from New Orleans.

"6. The towns of the interior are not in general affected till the disease has prevailed for some time in New Orleans.

"7. It never appears in any of the towns of the interior when it is absent from New Orleans.

"8. It generally begins near the wharves.

None of these facts can be questioned, but it is proper to estimate rigidly their value in reference to the theory which they seem to support, and this I shall proceed to do:—

"1. Ships are at all times arriving at New Orleans from Havana, Vera Cruz, and other southern ports of the Gulf; and, there-

fore, if the fever depended on a local or indigenous cause, developed in August and September, it could scarcely happen that the arrival of a ship would not be coincident with it.

"2. As to the absence of the disease before and during the war with England, it does not appear that it was absolute, for Dr. Hunestis, in his account of the diseases of Louisiana, and Judge Chamberlain, now of Mobile, but then of New Orleans, assure us that the fever occurred in 1812, after the declaration of war. But its complete absence during that period would not be conclusive, for in 1815 and 1815, the two years which succeeded the three years of war, it was absent; and in many towns, as for example, Mobile, notwithstanding an unrestricted intercourse with both Havana and New Orleans, it has been absent for periods of two, three, and even ten years; Natchez, from 1829 to 1837; Baton Rouge, in which it did not occur from 1829 to 1839, and again was absent to 1843; to which it may be added, that many towns in constant communication with New Orleans, were not visited till 1839; thus effectually destroying the conclusion drawn from the relative absence of the fever during the war.

"3. If it occur oftenest in the towns which have most intercourse with Havana; oftener, for instance, in New Orleans than Mobile, and in the latter, than in Pensacola, the introduction and agency of a ferment is not proved; for in proportion as towns have more shipping, they have more nuisances, and a more crowded wharf population, and therefore send up more of those exhalations on which the ferment is said to act.

"4. If the disease is almost confined to river towns, it may be because those exhalations which are to be fermented are there most copious.

"5. If the disease often shows itself soon after the arrival of a boat from New Orleans, it may be simply because a local influence is then matured. The previous arrivals without the supervention of the fever, show, according to the theory of ferments, that the local atmosphers was not yet impure enough for the alleged fermentation; when its impurity has become great enough for the fermentation, that process may or may not be necessary to an outbreak of the fever.

"6. If the towns of the interior are not in general affected till some time after the disease has prevailed, that may arise from a slower development of the required local atmosphere, and not from a necessary lapse of time for the production of the ferment in the atmosphere of the city. The conditions, including heat necessary to a local contamination, are less active in the smaller towns north of New Orleans than in that city.

"7. That none of the interior towns have it when New Orleans is exempt. This only proves that the causes which exempt that city, to wit., the absence, not of an imported ferment, but a local

impurity to be fermented, have been equally operative in the towns of the interior.

"8. If it generally appears in the neighborhood of wharves, it may be because the local contaminated atmosphere is there most concentrated.

"It appears then, that the facts admit of a different interpretation, and do not, therefore, establish the existence and importation of a ferment. But to take away the tendered proofs of a theory does not *disprove*, but merely leave it unsupported. In that condition I conceive the doctrine of ferments now to stand; and in the absence of the support by facts, let us inquire into its probability and necessity.

"1. Although I have called it a theory, we see that it is only an hypothesis, an assertion to be proved—an assumption to explain certain facts, which admit of a different interpretation; which, however, it must be granted, may not be the correct one. If, however, they can be differently explained, they can no longer be exclusively applied to the hypothesis to which they were referred, which before it can reclaim them must establish itself by other evidences.

"2. This hypothesis is not sustained by any known analogy, and, therefore, can derive no support from reasonings carried from the known to the unknown.

"3. It multiplies causes without having the necessity for them shown. If a peculiar impure state of the atmosphere is necessary to the action of the alleged ferment, it may be said that the disease arises from that atmosphere itself, independently of any transformation by fermentative action. This is only elevating a condition, which the other hypothesis admits, to the rank of a *sine qua non* in the production of the fever, the cause, and the *only* cause. It is substituting the impure exhalations themselves, before the alleged fermentation, for the products of that fermentation. Their existence is granted, indeed, demanded by the hypothesis of fermentations, for without them the ferments could not spread the disease; but the very existence of the ferment is an hypothesis. If *they* necessarily exist, whenever the fever occurs, they may be expected to produce some effect, for by the very terms of the case, they are impurities, and that effect may be—*the fever.*

"This brings us to the inquiry whether the theory of an indigenous atmospheric contamination can explain all the phenomena connected with the origin and epidemic prevalence of the disease, in doing which I shall take autumnal fever, a non-contagious endemic, for our model.

"1. Whatever may be the mode in which, or the medium by which yellow fever now spreads through a community, or appears successively in different towns, the first case of the disease cannot have been produced by morbid exhalation from the body of one

laboring under that disease, and must therefore be regarded as the product of a cause resulting from chemical action on materials composing the earth, or resting on its surface. But if one case could be originated in that way, others may, indeed must, whenever the same conditions exist. The same conditions may not, however, exist but at the spot where the first case was generated, and the disease may everywhere else depend on derivation from those who labor under it. But we have shown as a matter of fact that it is not thus propagated. We have also shown that many facts and arguments stand opposed to the hypothesis of ferments.

"2. If we can show that the disease has arisen independently both of contagion and of ferments, under circumstances which deteriorate the atmosphere with the material on which the ferment is said to act, we have made out a case identical with that which originally produced the fever, and proved that it is still, as at its primitive appearance, a disease of local origin, the offspring of certain conditions, and a necessary occurrence whenever and wherever those conditions exist.

"What, then, are the proofs of this origin?

"1. When treating of the origin of the fever at Pensacola, I have given the history of the generation of the fever on board of three national ships, the Natchez, Vincennes, and Hornet, altogether apart from any external influence; and when giving its history at Donaldsonville, I stated an instance of its being generated in a small boat on a voyage from Baton Rouge to that town, the disease not existing in either. I might greatly extend this catalogue if I choose to introduce cases of a less rigid character, for the number of instances of alleged spontaneous origin on ships, in the Gulf of Mexico, is very great, but I do not wish to employ apocryphal data.

"2. Passing by New Orleans, where the local and commercial conditions are too much mixed for a satisfactory estimate of the influence of either, I may refer to Mobile, as having several times been invaded, in such parts of the city, and under such circumstances, as to convince the whole of its medical men, and all its observing population, that the fever did, indeed, arise independently of importation. I may also refer to Natchez, some of whose epidemics began under circumstances which seem utterly adverse to the hypothesis of importation; likewise, to the country around Washington, where we have the authority of Dr. Branch and Dr. Monette, the latter a powerful advocate of importation, that the disease has occurred from local causes; to Apalachicola Bay, where, according to Dr. Hart, several cases occurred when no ships had arrived from a distance; to Fort Smith, where the disease appears to have prevailed in 1823; to Memphis, where it was epidemic in 1828, and as far as observation could guide those

who witnessed it, arose from conditions existing on the spot; finally, in our examination of the origin of the fever in all the times and places where it has prevailed in the Northern Basin of the Gulf, we found that in almost every case which admitted of a strict examination, the asserted importation of the disease could not be made out, but if it were not imported, it was of local origin.

"3. It does seem to me, then, that the fact of a local origin of some cases is as well established as medical facts generally are, and that we are at liberty to reason from it as a fixed *datum*. But does this prove that the disease is indigenous? The answer must be in the negative. For it is *possible* that in all these cases a portion of the ferment may have been introduced long before, and laid dormant for want of that element in the atmosphere on which it is said to act, and it is also possible that when such an atmosphere was generated the ferment took effect. We cannot disprove this, and therefore admitting all the facts which have just been cited, they do not demonstrate that the fever arose from causes entirely local. But although that grade of proof is not attainable, we may certainly approximate to it; for in the first place the very existence of such a ferment is, as we have shown, an assumption; in the second place, as it is represented to have been a product of the South, and as cold suddenly arrests the fever, it seems highly improbable that it could survive the action of several winters; and in the third place, it is contrary to all analogy that it should neither undergo spontaneous decomposition through so long a period, nor evaporate, nor be absorbed by the bodies to which it adhered, through a series of years, and thus be lost. If these reasons do not annihilate the claims of the doctrine of a ferment, it must, I think, be admitted that it reduces them to the very lowest degree of probability.

"4. The occurrence of the fever from local conditions, independent of any foreign agency, being established in even a single instance, the controversy is, *de jure*, at the end, for if it can be thus generated in one case, it may in all, and to say that it is not, is to superadd a hypothetical to a known cause, in violation of the rules of philosophizing. He, indeed, who admits its production in this manner in one instance, and denies it in others, is bound to support his denial,—he brings upon himself the *onus probandi*. When a phenomenon has been once proved to arise from a certain cause, true logic requires us to refer it whenever and wherever it may afterwards appear to the same cause. If then yellow fever on board the ship Natchez, or in the vicinity of Washington, or in the village of Memphis, arose from local causes only, it has arisen in every other town through which we have travelled, from like causes, in other words is indigenous.

"Having made this generalization, let us proceed to inquire

whether in its history it conforms to the laws of the endemic fevers with which we proposed to compare it.

"1. It prevails in a certain season of the year, setting in about the time the surface of the earth has attained its maximum heat, and ceasing when the temperature is reduced below a certain point.

"2. While like autumnal fevers it extends indefinitely to the south, it has, like them, shown itself limited to the north, but the limits of the two diseases are not the same, the boundaries of the former being much higher than those of the latter.

"3. Within these limits it invades certain localities and is never seen in others, and the same is true of autumnal fever. From some it is scarcely absent any year, while others are invaded only now and then, and the same is true of them.

"4. In some seasons, as those of 1819, 1827, 1829, and 1843, but above all in that of 1839, it appeared as a widespread epidemic, while in others it has been limited to a few places, or even to New Orleans; and autumnal fever varies in a similar manner.

"5. It is often seen to be influenced in its aspect by the cause of autumnal fever. The cause of typhoid fever has now and then produced modifications in its symptoms. In the year 1811, at Terre aux Bœufs and New Orleans, in the army, it was modified by scorbutus. During the prevalence of epidemic cholera, in the latter, it assumed many of the characteristics of that epidemic.

"The whole of which is true of autumnal fever.

"6. In successive years, at the same place, it presents diversities which cannot be explained; and in the same season, in different localities, variations of character have been noted; which is also the case with autumnal fever.

"Thus conforming to nearly all the laws of our non contagious, endemical, epidemic fever, it presents additional claims to be regarded as such. It meets in fact all the requirements of the theory of indigenous origin.

Is yellow fever, then, it may be asked, merely the remittent autumnal fever with which we are all familiar? The answer according to these views of its origin is, that it is one of the varieties, as a tertian fever is another. That it springs from a peculiar modification of the remote cause in the localities where it prevails; but that this modification is produced by local conditions, not by an imported ferment. That this variation in the character of the agent which produces it, is progressive. That the fever in June and July may wear the aspect and possess the intrinsic pathological nature of remittent autumnal fever, but in August, September, and October, display the more malignant character of yellow fever, returning in November to the milder form of an ordinary remittent.

"Such is the malarial theory of this formidable disease; that

which is held by a majority of the physicians of the South;—a theory which inculcates cleanliness and free ventilation in houses, yards, alleys, streets, boats, ships, and wharves, to obviate the development of civic malaria, which being prevented, the fever, according to this theory or according to the theory of an importation of ferments to act on that malaria, would equally fail to appear.

"But I cannot regard this theory as finally established, although obnoxious to fewer objections than the theory of ferments, for,—

"1. The whole pathological history of yellow fever *seems* to constitute it a specific fever, differing from an autumnal remittent too far to admit of its being a mere variety of that disease.

"2. It has occurred in localities where civic malaria can scarcely be admitted to exist, as in Washington and its vicinity, in the state of Mississippi; in Opelousas, and in the Navy Yard on Pensacola Bay, where a white sand drift constitutes the surface of the ground.

"3. It has been produced by exposure to the air, which has escaped from goods sent from a city where the dis ase prevailed. But I know of no facts which go to show that autumnal fever is ever produced in that manner. It is scarcely necessary here to repeat that there is no well-authenticated example of its spreading from this source.

"4. While the condition of a town to all visible appearance continues the same, the disease will appear and disappear, precisely as it does while the commercial intercourse with a place continues unchanged.

"Until these objections are removed, the cause of this disease should be kept *sub judice.* We are not compelled to choose among conflicting theories and declare that which is best supported to be established. It may be more probable than any other, and yet not be true."

Editorial Department.

CHLOROFORM IN CONVULSIONS OF INFANTS.

Dr. BROOKHART, of Harrisonville, Missouri, in a letter to us reports cases of convulsions in infants successfully treated by the internal administration of chloroform. In one child the convulsions occurred every three hours. Dr. B. had it immersed in a warm bath when a spasm was about to come on and gave it six drops of chloroform. The child was permanently relieved by the treatment. Dr. B. has treated successfully three other cases in the same manner.

MISCELLANIA.

Miss ELIZABETH PRATT, of Boston, has bequeathed $20,000 to the Massachusetts General Hospital.—Dr. LA ROCHE, of Philadelphia, has been appointed to the chair of Pathological Anatomy and Physiology in Memphis Medical College, to fill the vacancy occasioned by the resignation of Dr. O. T. QUINTARD, who designs taking orders in the Episcopal Church. We understand that Dr. LA ROCHE will not accept.—Dr. E. R. PEASLEE has again successfully performed the operation of ovariotomy. The tumor weighed twenty pounds.—The medical class of the University of Buffalo have passed resolutions complimentary to Professors HUNT and WHITE.—The students of the New York Medical College presented Professor BAKER with a very handsome speculum chair.—A Dr. GEORGE BLANCH was recently convicted, in Cincinnati, of manslaughter, for causing the death of a patient, by puncturing a popliteal aneurism.—Professor HARDY V. WOOTEN has retired from the Faculty of the Memphis Medical College.—HENRY ROSSIGNOL, M. D., has become associated with Professor L. A. DUGAS in the editorial management of the Southern Medical and Surgi-

cal Journal.—Dr. Bennett Dowler will not issue the Quarterly Journal, the circular of which we have already noticed, the publisher of the New Orleans Medical and Surgical Journal having made an arrangement with him to edit that periodical for a term of five years.—M. Geoffroy St. Hilaire, Professor at the Museum of Natural History, recommends bringing horse flesh into use as food. There is now a privileged slaughter-house for horses at Altort, near Paris, under the control of the Veterinary School where horse flesh is sold at twelve cents per pound. We were once invited to eat some horse steak, but there being beef steak on the table we chose the latter.—Dr. Austin Flint has retired from the Buffalo Medical Journal. It is now conducted by Prof. Sanford B. Hunt, M. D.—Diseases of Women and Children, by Gunning S. Bedford, M. D.; Atlas of Cutaneous Eruptions, by J. Moore Neligan, M. D., M. R. I. A.; The Practitioner's Pharmacopœa and Universal Formulary, by Jno. Foote, M. R. C. S., will soon be issued by S. S. & W. Wood of New York.

CIRCULAR.

TO THE PHYSICIANS OF KENTUCKY.

The undersigned having been appointed by the Kentucky State Medical Society a Committee to report on the "Statistics of Lithotomy and the prevalence of Calculous Diseases in Kentucky," earnestly solicits information on the following subjects:—

1st. What has been the number of cases of stone in the bladder, urethra, and kidneys in your region of country, as observed by yourself personally, or reported to you by reliable authority?

2nd. Are there any calculous disorders, that is, gravel and urinary deposits, in your district, and if so, what are the most frequent varieties?

3rd. What influence, if any, do age, sex, color, occupation, food, drink, modes of life, and geological formation, exert upon the production of the above diseases?

4th. Have you any reason to believe that the use of lime-water, malt-liquors, cider, wine, ardent spirits, pork, corn bread, hot

wheat bread, coffee, tea, and other articles of diet tend to favor the development of stone in the urinary organs and of calculous disorders.

5th. Is there reason to suppose that gout, rheumatism, intermittent fever, and other diseases exert any influence upon the production of these affections?

6th. How many operations have been performed for the removal of stone from the bladder in your region of country? Name the operators and their methods, together with the age, sex, color, occupation, and mode of life of the patient, the duration of the disease, and the results of the case; if successful, whether there was any relapse; if fatal, how soon, with the cause of death, and the post-mortem appearances. In either event, state the size, form, weight, and composition of the calculus.

7th. Have you noticed any cases of stone or gravel among the inferior animals?

As it is the intention of the undersigned to report at the next meeting of the Society, it is very desirable that the answers to the above queries should be sent as early as the 1st of October next. It is to be hoped that the numerous topics involved in them will be duly considered by every intelligent practitioner of the State. It is very important that the whole subject should be most thoroughly investigated; and it is obvious that this can be done, not by the efforts of a single individual, but by the united labors of many. Every physician of Kentucky, therefore, should consider himself under obligation to contribute whatever knowledge he may possess on the subject to the general stock. By ascertaining the etiology of these affections, we may, perhaps, ultimately counteract their formation, and thus prevent a vast amount of human suffering. Kentucky is the great centre of calculous diseases of the American Union; and no where, either in the New world or the Old, has the operation of lithotomy been performed with more brilliant success.

S. D. GROSS.

Louisville, May 15, 1855.

Record of Medical Science.

CASE OF PENETRATING GUN-SHOT WOUND OF THE HEART—LIFE PROTRACTED FOR ELEVEN DAYS—BULLET FOUND BURIED AND ENCYSTED IN THE SUBSTANCE OF THE HEART. By J. M. CARNOCHAN, M. D.—On the 27th of February, 1855, I was called in consultation to see William Poole, a young man aged 33 years, of unusually athletic form and muscular development, who had been wounded two days previously in an affray with fire-arms. He had received a bullet wound in the outer aspect of the right thigh, two inches above the upper border of the patella. The wound, however, which created alarm among his friends, was situated upon the anterior wall of the thorax, about three-quarters of an inch to the left of the mesial line, and about half an inch below a line drawn across the chest, from one nipple to the other. A bullet probe could be passed slantingly from right to left, along the track of the wound, for about an inch. At this depth, the probe was arrested, and it was not thought expedient to use force in making farther exploration. Poole received his wounds during a deliberate onslaught made on him by some five or six persons, armed with Colt's revolvers. The first ball took effect on the right thigh, and brought him to the ground. While thus prostrate, another assailant placed the muzzle of a pistol close to his chest, and discharged its contents. He immediately jumped up, and reeling towards a door, rested, as if stunned, against it for support, during some minutes. He then fell, exclaimed that he was dying, and remained senseless, cold, almost pulseless, and apparently moribund, for about four hours. From this condition he rallied, and became so free from the usual symptoms of severe injury, that his medical adviser, Dr. Putnam, considered that the ball had really not penetrated into the thoracic cavity, and my opinion was sought to corroborate or dispel this favorable view of the case.

I found him sitting up in bed, his back resting on pillows as a support, apparently at ease, and conversing with numerous acquaintances, who had come to visit him. His countenance exhibited no expression of anxiety, and he answered placidly and without effort the questions I put to him. His pulse was 80 in the minute, the respiration easy, the surface of the body normal in temperature and moist. The stethoscope revealed the existence

of no difficulty in the respiratory passages, and the normal *Tick Tac* of the heart beat with healthy precision. There were no signs of inflammation or of effusion into the pericardium.

With such freedom from morbid symptoms, I was disposed to concur with his medical adviser in auguring favorably of the case; for although it might be inferred from the external character of the wound, that the ball had passed somewhere into the cavity of the chest, it was not impossible that it had become lodged in some position, where it remained innocuous.

The previous treatment had been gently antiphlogistic; mild aperients, diaphoretics, acidulated drinks and low diet. The consultation resulted in a continuation of a similar mode of treatment, with the injunction, that he should be kept in a state of absolute bodily rest, and free from every cause of mental excitement, as I felt far from certain that he had not sustained mortal injury.

The symptoms in Poole's case illustrate in a remarkable degree some of the peculiarities of wounds of the heart, and also the assertion made by Hervey, that the heart is not very sensible. I am informed by Dr. Putnam, who saw him at one o'clock on the morning of the 25th, about fifteen minutes after the wound was received, that the patient was at first nearly pulseless, was insensible, and that respiration was performed with great difficulty. In this condition, laboring also under the ordinary signs of shock to the system from a gun shot wound, he continued for about four hours, before any signs of reaction were manifested. Vomiting now occurred; this was followed by increased action of the heart, and sensibility gradually returned.

During the same day (25th) he continued improving, and on the evening visit, the pulse beat 84. The skin was moist and natural, tongue healthy, with no unfavorable symptoms otherwise. No external hæmorrhage had occurred from the wound, nor had any evidences of internal hæmorrhage been evinced by vomiting or expectoration.

Monday, 26th. The wound was examined more particularly, and no traces of the bullet could be found, or any special indications manifested of its presence in the cavity of the thorax. Symptoms about the same.

Tuesday, 27th. I saw the patient for the first time, and found him in the favorable condition already stated.

Wednesday, 28th. Complained of slight headache, pulse 86, bowels not having been moved, a gentle aperient was ordered, by which the pain in the head was relieved. At times the patient had complained of transient and slight pain about the region of the heart.

Thursday, March 1st. Was called in to see patient a second time. Had slept well; pulse 80; respiration natural; appetite good; skin moist; action of the heart natural. He stated that he

felt no pain or unpleasant symptom except weakness, remarking, however, that he felt well enough to go out.

Friday, March 2d. The patient perfectly comfortable; pulse 82.

Saturday, March 3d. Patient so well that, upon visiting him, for the third time, by request, he was found receiving his friends, and, contrary to previous injunctions, conversing freely with them. Enjoined repose.

Sunday, March 4th. No untoward sign connected with either the functions of circulation or respiration. During the day, he received, against positive orders, the visits of more than a hundred people, with whom he conversed. His own statement was that he felt quite well.

Monday, March 5th. Dr. Putnam was sent for early in the morning. At eight o'clock, A. M., the patient was found in a high state of irritability; pulse 120; skin hot and dry, and complains of pain generally; respiration troubled and more frequent. An aperient was ordered, by which the symptoms were much alleviated.

Tuesday, 6th. Was again requested to see patient. Pulse 100; countenance anxious; the adnata tinged yellow; complained of debility, but said he had no pain about the heart; signs of effusion.

Wednesday, 7th March. Passed a restless night, notwithstanding the administration of an anodyne; pulse 120; countenance more anxious; respiration much troubled; inability to remain in the recumbent posture; symptoms gradually becoming more grave.

At two o'clock, A. M., Thursday morning, his attending physician was sent for. The patient was now rapidly sinking; pulse almost imperceptible, and with difficulty counted; respiration short, frequent, and difficult; extremities cold; countenance pallid and hippocratic. From this time he continued to sink, and expired, without a struggle, at five o'clock.

Autopsy seven hours after death.—The body was in a state of perfect preservation, and showed a powerful and well-developed organization.

The surface of the body presented three orifices of gun-shot wounds: two on the external side of the right thigh, a short distance above the patella, by which, apparently, a ball had made its entrance and exit, respectively; and one on the anterior aspect of the chest, three-quarters of an inch to the left of the median line, and about half an inch below a line drawn across the chest, from one nipple to the other. The examination revealed that all the organs of the body were in a healthy condition.

The sternum and cartilages of the ribs having been partially elevated, a bullet probe could be passed without difficulty, slant-

ing from right to left, through the wall of the thorax, at the place of junction of the cartilages of the fifth or sixth ribs with the margin of the sternum.

The sternum being completely elevated, the pericardium was seen to be much distended, and on its surface, in continuation with the external wound, was observed a rough spot, which proved to be an opening into the cavity of the pericardium, thinly closed by the exudation of plastic material.

The right and left cavities of the pleura were free from effusion, and the lung on each side was in a sound condition. The pericardium was found filled with serous fluid, tinged with blood, and was so distended that it encroached very much upon the lungs on both sides. Upon opening the sac of the pericardium, and removing the large quantity of serous fluid, the external surface of the heart and the serous lining of the pericardium were both found to be entirely covered with plastic exudation, presenting all over signs of high inflammatory action. A cursory examination of the heart in position did not disclose the presence of any foreign body. It was afterwards taken out, and upon a careful examination, a bullet one inch in circumference, was found enveloped in a delicate cyst, and imbedded, to the depth of a quarter of an inch, in the muscular tissue of the septum, between the right and left ventricles, about midway between the apex of the heart and the base of the ventricles. Its locality was only indicated by the sense of touch, for as the wound had entirely cicatrized, there was no outward visible sign of its presence. Obviously the cause of death was inflammation of the pericardium and heart and its results.

This case is one to be added to the few already on authentic record showing that penetrating wound of the heart are not always immediately mortal.

It has, moreover, peculiar features which will render it remarkable in the annals of surgical pathology.

Several cases are mentioned in which patients have survived one or more days the effects of penetrating and non-penetrating wounds of the heart, inflicted by cutting instruments, and also of non-penetrating wounds inflicted by *gun-shot*.

But the peculiarity of this case is, that although the wound was a penetrating gun-shot wound, leaving the ball deeply buried in the tissue of the heart, the patient survived for a period of time so long as to encourage the hope of recovery.

This position of the ball discriminates the case from that mentioned by the French surgeon, Latour, where the ball had not penetrated deeply into the heart, but rested on its surface, partially encroaching upon the muscular wall of the heart and enfolded partly by the pericardium.

The autopsy of this case also revealed, that the wound was not only closed and cicatrized, but that a cyst was in process of formation around the ball.

By this case, also, it is established, that hæmorrhage is not necessarily a consequence of a gun-shot wound of the heart; for the serum found in the pericardium was merely tinged with blood, and there was no coagulum. The absence of hæmorrhage may be accounted for by the conical shape of the ball, and by its direction, two circumstances which favored its passage between the muscular fibres of the superficial layer of the heart, without severing them, and caused it to rest slantingly behind the anterior coronary artery, without wounding it.—*The American Medical Monthly.*

Signs of Heart Disease.—

1. That cases of valvular affection may be divided into two classes, in one of which the disease has been produced by inflammation, while, in the other, it appears to arise independently of this condition.

2. That in the first class of cases, a period arrives in which, although the disease is progressive, there is no evidence of its being of an inflammatory nature.

3. That hence it is generally improper to persist in an antiphlogistic treatment of valvular disease beyond a certain period of time.

4. That the determination of the actual seat and nature of a valvular disease is of less importance than that of the vital and mechanical state of the heart.

5. That a permanently patent state of the orifices is the ordinary result of all valvular diseases. This condition may or may not be attended with contraction, or the orifices may be dilated.

6. That the period when inadequacy of the valves supervenes, varies greatly in different cases.

7. That hence, two series of phenomena may occur; in the first we have the signs of disorganization without inadequacy; in the second those of inadequacy are added.

8. That the distinctness of valvular murmur cannot be taken as being proportionate to the amount of disease.

9. That a complete cessation of murmur may coincide with the advance of the disease.

10. That the cessation of murmur, under these circumstances, has been only observed in connexion with contraction of the orifice; it has not been observed in cases of free regurgitation.

11. That absence of murmur does not necessarily imply absence of valvular disease, especially if there be symptoms of disease of the cavities.

12. That the number of cases in which we are warranted in making a special diagnosis of valvular disease is small.

13. That the number of pathological conditions competent to cause such changes in the valves as will produce murmur is very great.

14. That in the earlier periods of valvular disease, murmur may not occur, although the disease be progressive.

15. That even in chronic cases, the development of murmur may be sudden.

16. That the disorganizing process may advance with great rapidity, or with slowness, and that, in some cases, it appears to be really arrested.

17. That the irregular action of the heart is much more related to the state of the cavities than to that of the valves.

18. That we may observe the sudden development of the symptoms as well as of the physical signs of chronic disease of the heart.

19. That three conditions of the heart, considered in its vital relations, may accompany or follow valvular disease:—

1.—Increased force of the heart.

2.—Diminished force, with rapidity and irregularity of action.

3.—Diminished force, with remarkable slowness and comparative regularity of action.

20. That the law which regulates the production of the alteration of the cavities, which follows on valvular obstruction, with or without inadequacy, is still undetermined.

21. That considering the rarity of organic change in the valves at the right side of the heart, and the difficulty or impossibility of their special diagnosis, we may, in a practical point of view, limit our considerations to the diseases of the mitral and aortic valves.

22. That in the diseased and permanently patent condition of the valves of the pulmonary artery, a double murmur at the base of the heart, not propagated into the aorta, and not attended with general arterial throbbing, has been observed.

23. That in most cases of organic disease of the valves at the right side of the heart there is either an open foramen ovale, or a deficient ventricular septum.

24. That the most frequent result of disease of the right auriculo-ventricular valves is but the exaggeration of their natural insufficiency.

25. That we cannot by the ordinary acoustic tactile signs determine the existence of dilatation of the right auriculo-ventricular orifice.

26. That reflux pulsations in the veins of the neck, and occasionally in those of the upper extremities, indicate regurgitation into the right auricle.

27. That hence they may be taken as indicating the insufficiency of the valves, and may have, as their remote cause, morbid conditions of the pulmonary artery, the lung, or the left side of the heart

28. That of these different lesions the most frequent is contraction of the mitral orifice.

29. That the venous pulse thus produced may be permanently present, or only developed during an attack of cardiac asthma.

30. That the pulsations in the jugular veins are synchronous and isochronous with the ventricular systole.

31. That we must not depend on any acoustic character of the murmur, nor even on its exact seat, for the diagnosis of valvular disease. It is requisite to combine with these considerations those of the history and symptoms of the case, as well as those which have reference to the state of the pulse, the force of the heart, and the condition of the lung and liver.

32. That all diagnostics depending solely on the tone, character, and seat of murmur, are more or less doubtful.

33. That although by acoustic signs we may often determine the insufficiency of a valve, yet there are no means by which, from the stethescope alone, we can declare the cause of that insufficiency.

34. That the diagnostics between the contraction and dilatation of any of the orifices, founded on acoustic phenomena, are to be rejected.

35. That organic and anæmic murmurs may co exist.

36. That there are no distinctive symptoms of disease of the mitral valves, when it is uncomplicated with alteration in the vital or mechanical state of the cavities.

37. That its principal physical indication is a murmur which is systolic, but not propagated into the arteries, and loudest towards the apex and to the left side. This may or may not be attended with fremitus.

38. That the most common result of contraction of the mitral opening is pulmonary congestion, with enlargement of the right cavities of the heart.

39. That under these circumstances, from the preponderance of the right ventricle, a globular form of the heart may be produced.

40. But a globular form of the heart may exist with a dilated mitral opening, attended with enlargement of the left ventricle, while the right remains unaffected.

41. That the combination of a contracted state of the mitral opening, with permanent patency of the aortic valves, is of frequent occurrence.

42. That under these circumstances, we may occasionally observe both the aortic and the mitral murmurs

43. But that the absence of a mitral murmur, in a case of permanent patency of the aortic valves, does not necessarily imply that the auriculo-ventricular opening is free from disease.

44. That in cases of mitral contraction moveable coagula may be formed in the left auricle, which may, by occlusion of the opening, become a cause of sudden death.

45. That with the progress of contraction, the mitral murmur may gradually subside, and ultimately become extinct, so that with the increase of disease, we may have decrease and cessation of murmur.

46. That this cessation of murmur may coincide with a permanently patent though contracted opening.

47. That inasmuch as most cases of mitral murmur are systolic, they are to be held as regurgitant. We cannot by acoustic signs, distinguish between the direct constrictive and the regurgitant murmur.

48. That the interscapular murmur may attend constriction or dilatation of the mitral opening, but appears more allied to the latter than to the former condition.

49. That the interscapular murmur may be consequent on a recent and acute disease of the heart.

50. That the existence of a presystolic murmur, which theoretically should imply that it attended the passage of blood from the auricle into the ventricle, does not justify the diagnosis of absence of regurgitation through the mitral orifice.—*Stokes on Diseases of the Heart and Aorta.*

STRICTURE OF THE URETHRA.—This disease attracts considerable attention in the hospitals of Paris, and more especially has it done so within the last few years, during which time many important points in its pathology and treatment have been elucidated. Since the clear establishment of the point that organic strictures depend not upon a lesion of the mucous membrane of the canal, but upon the effusion and subsequent contraction of organizable fibrin in the corpus spongiosum, throughout the whole or a part of its thickness, the means for their relief have become at once more rational, and more efficient. In all cases dilatation by means of caoutchouc or wax bougies is first tried, and of course in a large majority of cases succeeds. The instrument is not allowed to remain in the canal, but is removed in from two ten minutes; one of larger size being each day employed until the desired dilation is produced. In cases which resist this treatment, incision from within outward, after the method of Reybard is adopted; and, in some few cases, incision through the entire body of the corpus spongiosum from without inward is practiced; cauterization, as advised by Prof. Lallemand,

is now universally rejected, and on all sides I have heard the assertion that after cauterization the stricture is always rendered more callous and difficult of cure than it was before.

In 1853, M. Reybard, who long ago introduced the internal section of strictures, published a work on the subject, and proposed an instrument consisting of a sound, bearing within it a concealed blade, which being introduced into the canal, the blade is protruded, and an incision into the corpus spongiosum effected, the work gained for him the prize of Argenteriel of 12,000 francs. Like all other innovations, the process has had, and still has, many detractors, and it certainly cannot be without danger, when we bear in mind the manifold risks from cutting into a venous plexus such as the corpus spongiosum; the proportion of deaths has, however, been very small, and out of numerous cases in which I have seen the method employed by M. Civiale during the past year, I cannot recall a single one which has terminated fatally. M. C. employs an instrument of his own invention, which is regarded as superior to that proposed by Reybard. As I have already stated, the operation of Syme, of Edinburg, consisting of an incision from one, to one and a half inches in length, through the corpus spongiosum, from without inward, upon a grooved sound in the urethra, is sometimes employed; thus M. Adolphe Richard has practiced it six times, and M. Sédillot four times—in Great Britain it appears to have gained many partisans. Mr. Syme has, since the beginning of 1844, performed it seventy times, McKenzie seven times, and Ferguson four times; while others, whose names are unknown to me, have brought the number to one hundred and fifteen. Out of Mr. Syme's seventy cases not one died; M. Richard out of six lost one; and, out of the entire number, only five deaths are reported—in each instance the fatal termination has been produced by purulent absorption. Previous to operating after Syme's method, M. Nélation, the distinguished surgeon of the Hospital of the Clinique, some days since, made some remarks on the subject, in which he expressed himself as not satisfied with the reliability of the statistics of this operation, and regarded it as much more dangerous than they would lead us to believe. During the year 1854, the operation was several times performed by Dr. Carnochan, surgeon of Ward's Island Hospital, New York, with entire success; but, as I possess no detailed notes of the cases, I can enter into none of their particulars.

As I am aware that these views are at variance with those long and generally received in America, I will state some of the authorities upon which I make, what may be otherwise considered, too bold an assertion; at the same time I will refrain from going at length into the subject, as its great interest and importance make me desirous of devoting to it, at some future time, a

separate article. Having experienced some difficulty in determining the present views of the following gentlemen, from their published works, and being desirous of avoiding even a chance of error, I called upon each of them, and was kindly furnished with the following information:—

M. Aug. Mercier informed me that although he does not deny the *possibility* of constriction of the canal at the prostatic portion, from great destruction and subsequent reparation of the prostate gland, he has never met with a case in his practice; and, in the membraneous portion, without disease of the bulb, (whose extremity extending under the commencement of the former may cause error,) he denies the possibility.

M. Malgaigne stated that, if strictures occur beyond the spongy portion of the canal, they must be very rare, for in his extensive practice he has never met with one in the prostatic portion, and the only time that he has been led to the belief that they could exist in the membraneous, was in a post mortem examination, when he found a constriction in that part, which appeared to him to resemble strictures in the spongy portion; this is the only case that he, in a very large experience, and paying particular attention to the point, has ever seen, and he regards the numerous cases reported as due to errors in measurement, which it is well known are sometimes ludicrously great.

M. Caudmont, a clinical lecturer of this city, who has for years made a speciality of this branch of surgery, denies, in a manner the most positive, the possibility of organic stricture below that portion of the urethra in contact with the corpus spongiosum. It is, however, upon the opinion of M. Civiale that I would lay most stress, and, at another time, I shall report it fully; although he does not state the point clearly in his work, he informed me not two days since, that he does not admit the existence of organic structure in any part of the urethra below the bulb; there is, says he, a state of muscular contraction which occurs in the membraneous portion when the bulbous is affected by stricture, and this has led to numerous errors. M. Ammussat, as quoted by M. Mercier, agrees with the opinion of Civiale.

Among the many subjects which offer themselves for investigation in Paris, there is none whose study is attended with more interest and profit than that of syphilis; each year's researches appear to be rewarded by new and important facts in its history, and, at last, it is being classified by clear and determinate rules, though it is true that these are, as yet, too recent to have overcome the opposition of those who, until of late, have been blind to their existence. Two grand classes of chancres have for years past been admitted, namely: the simple chancre and the specific or indurated; the first, is a purely local affection, and whether treated or not, whether heroically attacked with mercury or left to

itself, will get well without affecting the constitution with syphilis. The second variety, or the specific, is always, unless checked in its earliest infancy, followed by constitutional affection, and when once fairly fixed in the system, can only be relieved, and not cured by our art. The simple variety is known by the following well marked characteristics:—First, it is multiple; second, its border is not indurated; and, third, it gives a bubo which suppurates; and the specific is no less distinct in its symptoms—first, it is nearly always single; second, its border is indurated; and, third, it gives induration of the glands of the groin which *never* suppurate. All this is well known to you, but the point at which I wish to arrive is this:—Ricord believes that these two classes arise from the same poison, which is modified in its action by the constitution of the individual; thus if five men are exposed to the same contagion, three will contract specific chancres, while the other two will contract only simple ones, which all, under no circumstances, affect the constitution.

In opposition to these views, let me cite those of M. Clerc, an intelligent and conscientious investigator of this city; I give them from notes on his clinical lectures. M. C. believes that there exist two separate and distinct viruses, one of which, if successfully inoculated, will always produce a non-infecting chancre, and the other always an infecting one; in a person never having had constitutional syphilis, this will be the invariable result, the one kind no more producing the other, than the inoculation of small pox in an unprotected person will produce varioloid. Suppose, however, that the individual has already had constitutional syphilis, the predisposition is destroyed, and the inoculation, even of specific matter, will thereafter produce only a simple chancre, as in one protected by vaccine, the inoculation of variola will only produce varioloid. In his researches, M. Clerc follows this plan:—A patient presenting himself at his clinique, with a specific or non-specific chancre, as the case may be, he immediately takes the name and residence of the female who has communicated it, and arming himself with the presence of the police, he searches her out, and discovers the kind of chancre which she has. The results have, invariably, been in consonance with the fact, that chancres produce their like, the rule being no where broken, except where exemption has been purchased at the price of previous constitutional affection. To the specific variety he gives the name chancre, while to the other he applied that of chancroid. In support of the assertion, that to a constitution fortified by previous syphilitic affection, the inoculation of specific matter imparts only a non-specific sore, the extensive and well known experiments of Ricord appear; and there are, besides, numerous other instances on record. At the clinique of M. Clerc, there visits one of his friends, a physician who has had constitutional syphilis; he does not hesitate to allow M. C. to inoculate him with the pus

of specific chancres, and all the trials that they have made only produced a simple chancre, which soon gets well, and leaves no effect on the constitution.

In my last, I mentioned to you the experiments of M. Bazin, on Porrigo favosa, and his views as regards its vegetable origin, at the same time stating my inability to say in what light they were regarded by the profession. His success has been remarkable, and his views appear very generally admitted. Favus, Mentagra, and probably Herpes tonsurant, he also regards as due to a vegetable parasite, and, with slight modification, applies to them the same treatment. It appears that for a length of time a religious sect of France, called the "Frères Mahon," have possessed, and culpably kept secret, for their own aggrandizement, an effectual cure for this class of diseases, and it is highly probable that the physician of "St. Louis" has, by scientific researches, arrived at the same happy results, which they did by expiricism; actuated, however, by a more truly christian spirit, he has published his means of success freely to the world.

Before closing this letter, allow me to mention a fact of some interest in a pathological point of view, though probably in itself of no practical importance, on account of the rareness of its occurrence. The fact is one to which my attention was called in the service of M. Blache, at "Hôpital des Enfants Malades," namely: a paralysis of the veil of the palate, occurring after inflammation of the fauces, especially when that inflammation is of diphtheritic character. When the patient is in a state of convalescence, there is suddenly observed great difficulty of deglutition, more especially of liquids, which at each attempt regurgitate by the nostrils; at the same time there is dfficulty in pronunciation, and much to the anxiety of friends, the patient loses the power of expressing many words, while other words have that peculiar and disagreeable sound observed in persons suffering from cleft palate. The power of inflating the cheeks and of blowing is lost, and that which renders the affection grave in children, the act of suction is impaired, and sometimes lost. Examination of the palate shows traces of inflammation, redness and tumefaction existing; its contractile power is lost, and sensibility to such degree impaired, that a needle can be passed through the part without causing pain, and cauterization with nitrate of silver practiced without giving rise to any unpleasant sensation. Contractibility, under the influence of electricity, however, still exists, and the passage of a current shows distinct agitation and contraction in the veil and pillars. The prognosis is ordinarily so favorable, and the treatment so nearly negative, that the mere interest of the fact, as a pathological one, must pardon its perhaps useless insertion here, the more especially as my endeavor has hitherto been to confine my remarks to points of practical importance.—*Correspondence of the Charleston Medical Journal.*

Case of Injury to the Scrotum and Perineum. By Robert W. Gibbes, Jr., M. D., of Columbia, South Carolina.—Henry, colored man, aged 22 years, of robust constitution, and a workman at the foundry of J. N. Scoffield, while standing with one foot on a ladder and the other on a joist, engaged in adjusting a belt, on the 24th of July last, had his coat (which was buttoned closely in front) caught by the shafting, and his clothes and genital organs torn off.

I got to him about 8 A. M., which was within twenty minutes after the occurrence of the accident, and found that the following injury had been sustained:—The entire scrotum was torn off, with the skin and cellular tissue from two and a half inches above the spine of the pubis down to the edge of the sphincter and including all the breadth of the perineum, together with the left testicle with five inches of its cord attached, and all the integument and cellular covering of the penis, except a rim nearly half an inch wide at the extremity and continuous with the mucous membrane of the perpuce.

I picked up these parts (all in one piece) in a distant corner of the establishment where they were lying wrapped up in the cloths, and I have preserved the specimen in alcohol.

The patient was found lying on a table closely covered up with a blanket, (for his clothes with the exception of shirt, socks and neck handkerchief, had been all torn off), and suffering most acutely. Chloroform was immediately administered, and on examination the right testicle was seen hanging by its bare cord, and apparently covered only by the tunica vaginalis communis as high as the abdominal ring, where the elastic feel of the intestines was distinctly perceptible, as well as on the opposite side, where the end of the cord was so retracted as to render its ligation impossible.

I do not think more than a half ounce of blood could have been lost, as scarcely any oozing was observed, and the parts separated were almost as clean as if they had been sponged.

The gap in the perineum was brought together by three points of the interrupted suture, and, as it was impossible to form a covering for the raw surface and remaining testicle, a simple lint dressing was applied, the penis supported on the left thigh, and the testicle on a small bran cushion placed between the legs. The patient was then left, still under the influence of chloroform.

* * * * * * * * * *

October 1st.—The patient now begins to walk about; (a little more than two months after the accident) and it is very interesting to observe nature's slow, steady process of reparation. The testicle is high up in the right inguinal canal, and being entirely covered, resembles somewhat in appearance a bubo. The perineum is completely closed, very nearly up to the penis, which is

much diminished in length, and is gradually being covered by an apparently almost inextensible integument, similar, as might have been anticipated, to the cicatrix formed after a burn. The man, however, has occasional erections, and says that the organ swells out, stands nearly straight, and pains him but very little.

Not long after, this cicatrization being almost complete, the patient went out to his usual occupation.

January 28, 1855.—I have seen Henry to-day; the testicle, instead of resting as it did when I last saw him on the os pubis, can be felt very distinctly below and outside of the external ring. It is of about the natural size and appears by its weight to have pushed down the new skin and cellular tissue before it, thus forming for itself a thick and efficient scrotal envelope. The soft pubis has, as it were, descended or grown down upon the penis being continuous directly with the original mucous membrane of the prepuce, which last has, of course, lost its former characteristics and taken on those of skin; there being no prepuce now and the glans penis perfectly bare. The sexual appetite, he says, is as great as before the accident, and coition as agreeable as ever; the only difference he observes is that the semen "does not come as free." One of his fellow workmen said to me, "Oh he is as good a man as ever," to which Henry replied *that he believed he was.*

Though I have made every reference in my power, I have been able to find but two cases which bear any analogy to the above. In the Parisian Chirurgical Journal of Dessault for 1794, there is a case from Richter, translated into French from the German, by M. Brewer. A peasant's genital organs were torn off by his clothes being caught in a carriage wheel in the year 1782. The urethra and penis torn off as far as the prostate which "contused and torn, adhered by a few fibres and hung out of the wound." No vestige remained of either the right testicle or scrotum. Wound was dressed, patient bled and ordered a nitrated drink; pain and fever not very considerable; urine, which was suppressed for thirty-six hours, flowed involuntary and in great abundance; but the power of retaining it for six hours or even more soon recovered. On the fifteenth day the spermatic cord was divided near the abdominal ring; it as well as the testicle was found perfectly healthy. The prostate remained for some time so sensitive that the patient could not bear the slightest touch. At length, however, it got buried in the wound. After beginning to walk, any intemperance would occasion retention of urine which was relieved by purging. In ten weeks from the time of the accident, cicatrization was perfect. There remained an opening for the passage of the urine, three lines in diameter under symphisis pubis and a little to the left side.

The other case is related in Larrey's Memoirs of the Austrian Campaign of 1811. A soldier standing with his legs apart was

struck from behind by a bullet which carried away the following parts, viz:—The margin of the sphincter ani, skin of the perineum, the bulbous portion of the urethra, some of the skin of the scrotum and the right testicle—dividing its spermatic cord close to the ring and tearing the skin of the penis and prepuce. He was left for dead on the battle field, and only some days after he was carried to Reneveck Hospital in a very feeble condition. A good deal of disorganized substance had to be excised, which caused some hemorrhage, necessitating the ligation of several branches of the pubic artery besides that of the spermatic. Baron Larrey covered the left testicle under the portion of dartos that remained and brought the edges together. After some grave and tedious complications, a cure was effected at the end of four months—one of the most remarkable, says M. Larrey, with which he is acquainted. Unfortunately the observation is very incomplete.—*Charleston Med. Jour.*

Lectures on Clinical Surgery. By James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh.

Lecture iv. *Necrosis and Caries.*—When this man (S. R., aged 40,) was admitted to the Infirmary, ten weeks ago, his right foot presented a great degree of swelling; the parts were matted together, and there was a profuse discharge from several openings on the dorsal and plantar aspects. This state of things had resulted from injury sustained nine weeks previously by his treading upon a sharp hook, which lay with the points presenting upwards. The appearance of the foot was formidable, and the patient came here expecting that the removal of part or the whole of it would be necessary; whereas he has been relieved on much easier terms. The foot is now sound, and he is able to walk upon it. This case is interesting on its own account, and also on account of some important principles of surgical practice involved in it. What was the essential obstacle to healing? There could be no doubt it was seated in the bone, just as in disease of the elbow-joint, where there may be a great degree of swelling, and other appearances which might suggest to an experienced eye the idea that the soft parts were those chiefly concerned, yet soon after the removal of the diseased portions of bone, the soft parts acquire a healthy appearance.

The diseased bone might be either an exfoliation lying imprisoned amongst dense textures not admitting of its escape, or it might be in the state of caries. With reference to the question, which of these two conditions was present, we must particularly consider the nature of caries. Caries is often spoken of as equivalent to an ulcer of the bone, but this is not correct, for you may

induce an ulcer in bone, which will heal readily—*e. g.*, the spongy bone which is divided in excision of the elbow-joint assumes afterwards the same condition as an ulcer of the soft parts, yet it heals without any remarkable obstinacy, which is the chief characteristic of caries. When a joint is opened by a wound, if union by first intention do not occur, inflammation with suppuration necessarily follows, but caries may or may not result. A gentleman of my acquaintance once removed a loose cartilage from the knee of a patient. Inflammation of the joint came on, with suppuration, which was very profuse when I saw the case, but recognising some points about it that led me to think favorably, I recommended longer delay, and the discharge ultimately dried up. In one of the cases in which you saw excision of the elbow-joint performed, there was anchylosis resulting from injury, the articular cartilages had been removed during the suppurating process, yet the osseous surfaces were firmly united, which they could not have been if caries had existed between them. Pressure, it is said, will cause caries, but this is a great error; tumors pressing on bones may cause interstitial absorption—*e. g.*, an atheromatous tumor, pressing upon the frontal bone, may produce a depression on its surface, which however still remains covered by its periosteum; still further, the pressure of an aneurism may cause the absorption of large portions of the vertebræ, or of the whole thickness of the sternum, through which the tumor may protrude beneath the integument, or again of half the clavicle. This is not caries however; and no one can doubt, that if the aneurism were cured, the bone would heal. It is said, that pressure produced by matter causes caries; but this is a very erroneous and mischievous opinion. When matter is formed in the interior of a bone, as, for instance, in connexion with an exfoliation, the pressure of the matter causes openings in the surrounding bone; but no one supposes that the edges of these openings are carious. How, then, is the caries produced? Inflammation, it would seem must necessarily precede it, and it depends upon the condition of the part, or the constitution of the patient, whether caries will follow or not. This, you will say, is very unsatisfactory; but it is as far as we can go at present. And I beg to remind you, that there is no more certrin step towards the attainment of truth, than becoming conscious of our own ignorance. This is a dark point in pathology, which it is not impossible some gentleman now before me may be destined to clear up; and, meanwhile, all that we can do as practical men is to consider the circumstances that favor the production of caries. In the first place, the age of the patient appears to have great influence in determining the result of inflammation of bone; the younger the patient, the less is the risk of inflammation followed by suppuration going on to caries; for I need scarcely say, that if there be no suppuration, there is no

caries. Hence the success of certain operations which have been performed on young patients almost infants, which, if tried on older persons, would probably have met with less satisfactory results; the older the patient, the greater is the risk of caries, and the chances of its occurrence are also greater the more unsound the constitution, or, as is generally said, the more scrofulous the constitution. The term scrofula in generally understood to imply a specific tendency depending upon hereditary taint; but the expression weak or unsound constitution, is probably, in reality, nearer the truth. Give me the strongest child, and I could undertake to make him scrofulous by bringing him up in a damp cellar, with deficient food, and imperfect clothing. No doubt this state of constitution may be transmitted from the parent; but it may also as certainly be acquired. If the suppuration is the result of a wound, the danger of caries is much smaller than if it has arisen from some injury other than a wound—such as a twist or sprain. It is impossible for you to be too much impressed with this fact. More than twenty years ago a young man, employed by a butcher at Leith, was standing on a bench putting a joint of meat on a hook above him, when the bench slipped, and, as he fell the hook pierced his wrist, entering at the palmar aspect, and passing obliquely through the carpus almost to the back of the hand. He was afterwards sent to Minto House, which was then occupied as a surgical hospital under my care. There was a great degree of inflammation about the wrist; but being at that period aware of the distinction which I am pointing out to you, I said, that as the suppuration was the result of a wound, it was right to give the limb a chance. Free vent was, therefore, given to the matter by incision, but no improvement followed; and, at the end of six weeks, the discharge was profuse, and grating was felt when the bones were moved. Amputation was now proposed; but the patient expressed a wish to go home for a short time, and try, as a last resource, the effect of change of air. He did so, and returned before many weeks were over with his hand perfectly well.

I might mention many cases, but will content myself with two others that appeared to me peculiarly illustrative. A groom ran a penknife accidentally into his wrist, inflammation and suppuration followed, and he was brought to this infirmary. Within a day or two, a female patient was admitted, with swelling of the wrist and fluctuation, resulting from a sprain. In speaking of these two cases in this theatre, I expressed the opinion that in the case of the man, as the derangement had resulted from a wound, it might perhaps be cured; but that the woman's wrist, although less swollen, having had a different origin, would probably prove incurable. Both were treated alike, free vent was afforded to the matter, both were placed in splints, and both dressed with a lotion.

For awhile, the only difference between the two cases was, that the appearance of the man's wrist was much the more formidable; but, after a time, improvement began in him, and he went on to a perfect cure. But, in the woman's case, after the disease had remained apparently stationary for a while, the pain increased, the discharge became more profuse, her health began to suffer, and at length we acceded to her request, and amputated the limb. It is peculiarly incumbent on young practitioners to wait and allow time for the case itself to declare what its issue will be. If the disease has originated in a wound, and the patient is young and of good constitution, you will no doubt think more favorably of the case than if the opposite conditions were present; but I would advise you to be slow to act upon any of these modes of guidance, and to trust to time to decide for you; while the treatment which you will adopt, in the meantime, will be to allow free drain to the matter, to keep the limb, as much as possible at rest, and to attend to the general health. In the patient before you, the disease, as I before mentioned, had originated from a wound, a hook having pierced the foot, and caused inflammation and suppuration. On examination with the probe, I found that the disease, instead of affecting all the bones of the tarsus, was confined to the joint between the second toe and its metatarsal bone; and it further appeared that some small exfoliations lay loose in the middle of the foot. I removed these, and they proved to be, as you see, thin shells of dense bone; one of them, which is convex, corresponding to the rounded end of the metatarsal bone, and the other, to the concave articular surface of the phalangæal bone. The dense bone had died from the inflammation excited by the injury, while the loose osseous texture adjacent had ulcerated sufficiently to throw off the dead portions, but was not carious, for, since the removal of the exfoliations, the swelling has gradually diminished, the discharge has almost ceased, and he can bear his weight upon the foot, and recovery may be said to be complete.

There is another case to which I will now call your attention, as the same principles are concerned in it; it is one of sinuses about the pelvis, presenting some similarity to that of T. C——, from whom I lately removed an exfoliation of the rami of the ischium and pubes. The patient is J. U——, aged twenty-five, obviously of a very weak and unhealthy constitution, employed in a silk mill at Congleton, in Cheshire, where he was sometimes engaged for three hours together in lifting from the floor to the level of his head the wooden rollers used in silk-carding. Two weeks after last Christmas (1853) he began to suffer great pain in the region of the tuberosity of the left ischium, and a small lump about as large as a pea appeared on the back of the thigh, an inch or two below that prominence. The lump increased in size

and the pain became so severe, that he was obliged to give up work. Poultices were ordered by his medical attendant, and soon after their application the tumor broke, and discharged a large quantity of matter; the surgeon also lanced another place near the first, and a third opening subsequently formed lower down on the back of the thigh. A profuse discharge has continued ever since from these apertures, and he has become much emaciated. He was recommended to seek my advice, and came to Edinburgh accordingly, and was admitted to the infirmary on the 25th of August. At this time I enlarged one of the sinuses sufficiently to admit my finger, and found that it led up towards the pelvis, but could not discover any exfoliation. He continued to lie here without much change for two months, when I determined to explore the sinuses still more thoroughly; and he was brought into this theatre, as you will remember, and chloroform having been administered, I enlarged the highest opening freely with a probe-pointed bistoury, and on introducing my finger, discovered a large part of the surface of the tuberosity of the ischium bare and rough, but firmly fixed. Considering the length of time that the disease had existed, it appeared to be probable that it was caries; but I thought it best to let the patient lie quiet for a few weeks, when if there were an exfoliation, it would be either loose or so far detached that we should be able to recognise it as such. Four weeks have elapsed, and I propose to examine the man again now; but before he comes, let us consider the course we must take if, as I expect, we find the bone carious. He is losing flesh and strength. Can anything be done for him if the disease is of that incurable kind which we call caries? If it is limited to the tuberosity of the ischium, I see no objection to cutting out that part. It may be said that, so far as I know, no one has ever yet performed such an operation, but that is no reason at all why we should not; if we are satisfied that the operation is expedient and practicable, the mere absence of precedent is no obstacle. The tuberosity of the ischium may be reached without risk of injury to any important parts, and the mere division of the attachment of the muscles is of no moment, for we know that, as after excision of the elbow-joint, the muscles will acquire fresh attachments. The operation is therefore practicable, and it is also expedient, for we can make sure of removing the whole of the disease. In this respect the present case is very different from those in which the hip-joint is the part affected; there it is of no use to attempt removal of the disease, for it cannot be completely taken away, since the acetabulum is not only affected, but is generally more diseased than the head of the femur. You will see persons in the streets with one leg shorter than the other, and the toes everted or inverted, as the case may be, moving on with a greater or less degree of lameness; these are people who have

For awhile, the only difference between the two cases was, that the appearance of the man's wrist was much the more formidable; but, after a time, improvement began in him, and he went on to a perfect cure. But, in the woman's case, after the disease had remained apparently stationary for a while, the pain increased, the discharge became more profuse, her health began to suffer, and at length we acceded to her request, and amputated the limb. It is peculiarly incumbent on young practitioners to wait and allow time for the case itself to declare what its issue will be. If the disease has originated in a wound, and the patient is young and of good constitution, you will no doubt think more favorably of the case than if the opposite conditions were present; but I would advise you to be slow to act upon any of these modes of guidance, and to trust to time to decide for you; while the treatment which you will adopt, in the meantime, will be to allow free drain to the matter, to keep the limb, as much as possible at rest, and to attend to the general health. In the patient before you, the disease, as I before mentioned, had originated from a wound, a hook having pierced the foot, and caused inflammation and suppuration. On examination with the probe, I found that the disease, instead of affecting all the bones of the tarsus, was confined to the joint between the second toe and its metatarsal bone; and it further appeared that some small exfoliations lay loose in the middle of the foot. I removed these, and they proved to be, as you see, thin shells of dense bone; one of them, which is convex, corresponding to the rounded end of the metatarsal bone, and the other, to the concave articular surface of the phalangæal bone. The dense bone had died from the inflammation excited by the injury, while the loose osseous texture adjacent had ulcerated sufficiently to throw off the dead portions, but was not carious, for, since the removal of the exfoliations, the swelling has gradually diminished, the discharge has almost ceased, and he can bear his weight upon the foot, and recovery may be said to be complete.

There is another case to which I will now call your attention, as the same principles are concerned in it; it is one of sinuses about the pelvis, presenting some similarity to that of T. C——, from whom I lately removed an exfoliation of the rami of the ischium and pubes. The patient is J. U——, aged twenty-five, obviously of a very weak and unhealthy constitution, employed in a silk mill at Congleton, in Cheshire, where he was sometimes engaged for three hours together in lifting from the floor to the level of his head the wooden rollers used in silk-carding. Two weeks after last Christmas (1853) he began to suffer great pain in the region of the tuberosity of the left ischium, and a small lump about as large as a pea appeared on the back of the thigh, an inch or two below that prominence. The lump increased in size

and the pain became so severe, that he was obliged to give up work. Poultices were ordered by his medical attendant, and soon after their application the tumor broke, and discharged a large quantity of matter; the surgeon also lanced another place near the first, and a third opening subsequently formed lower down on the back of the thigh. A profuse discharge has continued ever since from these apertures, and he has become much emaciated. He was recommended to seek my advice, and came to Edinburgh accordingly, and was admitted to the infirmary on the 25th of August. At this time I enlarged one of the sinuses sufficiently to admit my finger, and found that it led up towards the pelvis, but could not discover any exfoliation. He continued to lie here without much change for two months, when I determined to explore the sinuses still more thoroughly; and he was brought into this theatre, as you will remember, and chloroform having been administered, I enlarged the highest opening freely with a probe-pointed bistoury, and on introducing my finger, discovered a large part of the surface of the tuberosity of the ischium bare and rough, but firmly fixed. Considering the length of time that the disease had existed, it appeared to be probable that it was caries; but I thought it best to let the patient lie quiet for a few weeks, when if there were an exfoliation, it would be either loose or so far detached that we should be able to recognise it as such. Four weeks have elapsed, and I propose to examine the man again now; but before he comes, let us consider the course we must take if, as I expect, we find the bone carious. He is losing flesh and strength. Can anything be done for him if the disease is of that incurable kind which we call caries? If it is limited to the tuberosity of the ischium, I see no objection to cutting out that part. It may be said that, so far as I know, no one has ever yet performed such an operation, but that is no reason at all why we should not; if we are satisfied that the operation is expedient and practicable, the mere absence of precedent is no obstacle. The tuberosity of the ischium may be reached without risk of injury to any important parts, and the mere division of the attachment of the muscles is of no moment, for we know that, as after excision of the elbow-joint, the muscles will acquire fresh attachments. The operation is therefore practicable, and it is also expedient, for we can make sure of removing the whole of the disease. In this respect the present case is very different from those in which the hip-joint is the part affected; there it is of no use to attempt removal of the disease, for it cannot be completely taken away, since the acetabulum is not only affected, but is generally more diseased than the head of the femur. You will see persons in the streets with one leg shorter than the other, and the toes everted or inverted, as the case may be, moving on with a greater or less degree of lameness; these are people who have

had inflammation and suppuration of the hip-joint, so that recovery is no proof that the operation was necessary or expedient. I incline to attribute the recovery in these cases to the fact that the patients have generally been young, and taken from the poorest classes, and have had every attention lavished upon them, wine and cordials, and delicate articles of food being freely given them, and then, in their convalescence, they have been sent to Margate or Brighton, or elsewhere, for change of air. It may, perhaps, still be said that cutting out the head of the femur may promote recovery, and make it more complete; I say, that if anchylosis occurs, the limb remains more or less useful; but if you cut out the head of the bone, it can never be of any good as a support to the body; it will be just as immediately after fracture of the neck of the femur, or as if the head of the bone were congenitally absent; the patient may carry the limb, but the limb can never carry him.

[The man was now brought into the theatre, chloroform was given, and Mr. Syme, on repeating the exploration of the sinus with his finger, found the diseased bone still firmly fixed, and with the same bare, rough surface as before. Mr. Syme mentioned these facts to the class, and expressed his intention of removing the disease. Next day the patient was again put under chloroform, and turned round upon his face; Mr. Syme then inserted a curved, probe-pointed bistoury into the large sinus situated at the inner part of the natal fold, and cut freely upwards through the mass of the buttock, so as to expose the posterior surface of the tuberosity of the ischium completely. Finding that the disease did not involve the whole tuberosity, but was limited to its posterior aspect, he removed the carious parts with the bone-pliers till nothing but perfectly sound bone remained. Two small vessels alone required ligature. A piece of lint was placed on the wound, and the patient taken back to bed. On examination of the piece of bone removed, the diseased part presented the ordinary appearance of caries, which involved the greater part of the posterior surface of the tuber ischii, and penetrated rather more than half an inch into its substance.

On the following day, Mr. Syme, in speaking of the case in his lecture, pointed out the similarity between this man's employment and that of the first patient, from whom he removed an exfoliation of the pelvis, who, just before the commencement of his symptoms, had been engaged in curing herrings, alternately stooping down to lift them from the ground, and stretching up to the full extent to place them above the level of his head. In that case the extraordinary strain upon the flexor muscles had caused inflammation at their origin, which had led to an exfoliation from the tuberosity of the ischium; in the present case, the patient, as will be remembered, was frequently engaged for three hours together

in taking up silk carding rollers from the floor and placing them at the level of his head, but the inflammation excited in the tuberosity of the ischium had in this case ended in caries; the difference between the results in the two patients probably depending on difference of constitution. No bad consequences followed the operation, and the discharge, which had previously been very profuse, became proportioned to the wound in the soft parts, which has been steadily contracting.]

Lecture v. *Fracture of the Skull.*—This young man, John W——, aged twenty-three, was at work at a flour-mill in Kirkaldy on the 25th of July last, when a fourteen pound weight fell from the fourth story, and struck him on the head, about an inch and a half from the vertex, where you now observe a depressed cicatrix. On his admission here, I found a depressed fracture of limited extent, and, on removing with bone-pliers the fragments of the outer table, discovered a circular portion of the inner table, of much larger size, which I succeeded in extracting piecemeal through the opening in the outer table. He was conscious at the time, and has ever since been in the full enjoyment of his intellectual faculties. The wound was simply dressed, and went on favorably till its contraction produced inversion of the edges of the scalp, when the hairs growing into the wound prevented further cicatrization, till I pared away the inverted edge a few weeks ago, after which the wound soon healed completely.

In such cases as this there is no reproduction of bone in the central part of the cicatrix, which is intimately adherent to the dura mater; and though it becomes after a time exceedingly dense, yet it is prudent for the patient to wear some sort of protection if liable to be exposed to external violence.

I may take this opportunity, gentlemen, of remarking that injuries of the head have long been a fruitful subject of bad practice and traditional error. In the olden times injury of the head, accompanied with insensibility, was thought to require scalping of the part, so as to allow a thorough examination of the bone, when if a fissure was discovered, the trephine was applied, under the idea that noxious transudations were liable to pass from without inwards. Even in cases where there was no evidence of cerebral disorder, it was the practice to remove any flap of the scalp that might have been torn up from the cranium. So lately as the time of Charles II., we find it mentioned in the valuable cases recorded by Wiseman, then sergeant-surgeon, that a surgeon who had cut off a large flap of skin in these circumstances was so proud of his performance, that he hung up the piece of scalp in his shop, to show what a great operator he was. Even in recent times, whenever injury of the head produced loss of consciousness, it was the practice to bleed largely immediately after the accident.

All these errors have now been disposed of, but something still remains to be cleared away. You have, no doubt, all of you heard of the operation of trephining, and have read that it is required under the following circumstances:—1st, When purulent fluid exists between the bone and the dura mater; 2nd. When blood has been effused from the main artery of the dura mater; 3rd. When the bone is depressed injuriously: and you will perhaps be somewhat surprised when I tell you that, if you except the case of punctured fracture of very small size, such as a sharp-pointed instrument would produce, trephining is never of any service. Take first the cases of effusion of matter and of blood. Though I have been now nearly thirty-five years connected, in one way or other, with this hospital, I have never known a single case where blood or matter has been withdrawn with a satisfactory result; indeed, I have never seen blood withdrawn at all by trephining, nor did I ever find a surgeon who had met with such a case. You will therefore do well to regard trephining under these circumstances as an operation that does not at all concern you. Also in depressed fracture, if you except the special case of small puncture, you will find trephining never necessary. If the bone is broken, it is so much so that it can be elevated by bone-pliers. Make a crucial incision in the scalp, and raise the flaps; try different parts with the bone-pliers till you can raise a small portion; cut off bits of the overhanging edge, if necessary, and you will be able to complete the elevation of the whole. If the bone has not injured the dura mater, the patients generally recover; if the longitudinal sinus is injured, they die, so far as my experience goes, although there appears no special reason why injury of this venous channel should be so much more serious than that of others, and cases of recovery have been recorded. If the dura mater has been injured so as to expose the brain, the case is very unfavorable; and if the brain itself is injured, it is almost hopeless, I say, *almost* hopeless, for it is not entirely so. A little more than a year ago a fire occurred in the Cowgate, which, as you know, is an old street of lofty houses. The occupiers of the fourth story threw out their valuables into the street, and, amongst the rest, a child four years old. It appeared to have fallen on its head, and had injured one parietal bone, over which a soft tumor existed. One day passed, and next day it appeared that some epileptic seizures had occurred, which, on the following day, became almost continuous, the child being scarcely out of one fit before another came on. I thought it right to open the tumor, and on my doing so, blood issued, and a wide crack appeared in the parietal bone, which had given exit to a quantity of cerebral substance, the appearance altogether being so frightful that I did not complete the incision, but regarding the case as hopeless, sent the child to bed with a piece of wet lint on the wound. However,

the convulsive attacks ceased from that time forward; the child showed signs of intelligence within two hours; on the third day it was sitting up, with a smile on its countenance; and in a fortnight after the operation it was running about the ward. I learned a short time since that this child was in the most exuberant health. But such cases are unfortunately quite exceptional.

I may remark, as we have happened to speak on this subject, that epileptic attacks sometimes come on months or years after injuries of the head; and you may perhaps be called upon to "do something for the patient," as it is said—that is to say, to trephine the skull. I have myself on several occasions exposed the bone and removed portions in such cases; but, from the results which have followed, I advise you positively against such a course. The The case is different, however, if a sinus continue in connexion with a sharp piece of bone pressing on the brain. I once operated on such a case, and some years afterwards had the satisfaction of seeing the patient in perfect health. The case is thus reported in the Tenth Report of the Edinburgh Surgical Hospital for 1833, (*Edinburgh Medical and Surgical Journal*, No. 115.)

Wm. K——, aged twenty-two, from Dunfermline, was admitted on the 3d of September. Between six and seven years ago he suffered a very severe wound of the face from the blasting of a rock. He was at first insensible, and considered in a hopeless state, but gradually recovered so as to be able to follow his employment as a weaver. A small opening, however, between the cheek-bone and ear continued to discharge matter, and he occasionally suffered much pain in his head. During the last four months he had had occasional epileptic fits. On examination, it appeared that the malar bone had been fractured, and the whole upper part of the face was occupied by a dense cicatrix. A piece of bone was felt at the bottom of the sinus, bare, and lying obliquely in respect to the surface of the skull. On the supposition that this was a portion of the cranium which had been depressed at the time of the injury, a crucial incision was made through the cicatrix, to expose the aperture of the skull, which was then enlarged by means of cutting-forceps, so as to admit the extraction of the loose piece. It comprehended both tables of the bone, (temporal,) and was about a third the size of a sixpence. The patient expressed much relief after the operation; but the fits still recurred, though seldom, and a profuse discharge issued from the wound. A probe being gently introduced, entered to the depth of two inches perpendicularly, the substance of the brain seeming to have been hollowed out into an abscess. He returned home, and continues in the same state, though, on the whole, improved.—*London Lancet.*

HIP DISEASE.—The affections which bear more or less resemblance to hip disease are, psoas abscess, caries of the pelvis, inflammation of the sacro-ilias articulations, chronic rheumatism of the hip-joint, sciatica, and hysterical affections of the joint. You are already aware how, in the early stage, hip disease seldom presents any other symptoms than pain and some slight impediment to motion, and hence you can see at once that nervous disorders are the most likely to simulate this first stage of the disease, because their characteristic sign is pain, and the pain, if severe, entails more or less difficulty of motion. What, then, are these painful nervous disorders? They are sciatica and hysterical affections of the joint. Sciatica is chiefly characterized by pain, so is hip disease, at the commencement, but there are several circumstances by which the two complaints can be distinguished without much difficulty. The pain in sciatica becomes very severe soon after the onset of the disease, and though it may be first felt about the hip it soon extends down the thigh to the leg, following the course of the sciatica nerve and its branches. In hip disease, as I have told you, the pain as often commences in the knee as in the hip, perhaps oftener; it is seldom very acute at first, and artificial motion during this early stage never produces the violent aggravation of pain which is excited by moving the limb during a severe attack of sciatica. Again, patients affected with hip disease can go about tolerably well during the first stage, showing slight lameness only, whereas a person attacked by sciatica is soon laid up and quite unable to place the foot on the ground, much less bear the weight of the body. As the diseases progress their distinguishing symptoms become much more clear and decided. After the first smart attack, sciatica has a tendency to diminish, even though no treatment be employed; mere rest alleviates the pain, and in a week or two a manifest remission takes place, whereas the progress of hip disease is steady and onwards; and during the second or third stages no error of diagnosis can be committed.

Hysterical affections of the joint simulate incipient hip disease much more closely. Like sciatica, these affections are characterized by intense pain in the neighborhood of the joint, together with a real or imaginary inability to move the affected limb; but on carefully examining all the circumstances of the case, you will seldom experience any difficulty in arriving at an accurate diagnosis. In the first place, the patients are almost invariably females; and whenever one of the fair sex complains of intolerable pain in a joint, with little appearance of organic lesions beyond a mere puffiness of the integuments, you may be pretty certain that her complaint is hysterical, for it has been calculated that at least four-fifths of the females amongst the higher classes of society, supposed to be suffering from diseases of the joints, are,

in reality, affected with hysteria, and nothing else. You will not, however, rest content with this general inference, but direct your attention to the concomitant signs of hysteria, and compare any existing symptoms with those that are known to attend true hip disease at a corresponding stage. It is an established fact, that hysterical females are more subject than any other individuals to neuralgia, and it seems to me highly probable that the complaint now under consideration is hysterical neuralgia of the joint. Many of the characters of the pain lead to this conclusion. It is excessively violent from the commencement, and in this respect differs from the pain of true morbus coxarius. The patient also refers it to the hip-joint, and not to the knee, as is so often the case when the disease depends on incipient organic changes. The pain, again, is not limited to the affected articulation, but extends to the buttock and down the thigh, a circumstance in which it resembles sciatica. This last affection, indeed, is often nothing but rheumatic neuralgia. This general diffusion of the pain constitutes an useful distinction between the hysterical complaint and true disease of the joint. The irregularity of the pain, and the peculiar modifications of sensibility which accompany it, are highly characteristic, and at once point to an hysterical origin. The pain, though apparently intense, is not constant; it often suddenly disappears for a time, without any evident cause, particularly if the patient's mind be diverted, from her condition; and in like manner, the more the sufferer's attention is directed to the part, the more she complains and appears to suffer. Besides this, the pain is absent during sleep, whereas the rest is always more or less disturbed by the pain of organic disease. The sensitiveness of the skin is truly hysterical; the patient shrinks from the approach of the finger, yet sometimes will allow you to press the joint in all directions, without complaining of much pain. The nates and the limb in general are not wasted, and there is no elongation of the limb. Sometimes, however, from the spasmodic muscular contractions to which these hysterical patients are very liable, the limb is drawn up and inverted, the trochanter major is prominent, the knee bent, and the thigh rests on the sound one. Here the case might, by a superficial observer, be easily mistaken for dislocation; but its history, and the possibility of bringing the affected limb into a natural position, will distinguish the case from one of true displacement of the bone.

The foregoing are examples in which functional disorders simulate organic disease in its early stages; in other cases, the difficulty of diagnosis may depend on the existence of some organic lesion in or near the joint, yet differing in its nature from the changes which constitute true hip disease.

It is evident that the more nearly these lesions resemble in their nature those of morbus coxarius, and the closer they are to the

joint, the more numerous will be the points of resemblance between the two affections. Experience proves this to be the case, and shows that caries of the pelvis, inflammation of the sacro-iliac symphysis, and psoas abscess, are the affections most likely to be confounded with chronic hip disease. When caries affects any portion of the pelvis close to the hip-joint, the functions of the articulation are impeded; pain is felt in the region of the joint, and abscesses form in its neighborhood; but the general symptoms are always less severe than when the articular cavity is the seat of the disease; the pain, also, is less severe, and not so evidently located in the joint, the movements of which are not very materially impeded. In hip disease, the immobility of the limb, after a certain period, is a constant and characteristic symptom; whereas here, although the hip-joint may be stiff, and a disinclination to movement exist, certain motions can be affected, without inducing any great pain. The suppuration from caries of the pelvis is less abundant in most cases than the discharge from hip disease, and after some time the probe will detect the existence of caries in the bone, when all doubt as to the nature of the disease will be removed.

Chronic inflammation of the sacro-iliac symphysis often bears a close resemblance to morbus coxarius. In children, a true joint, furnished with its synovial membrane, exists at the union between the sacral and iliac bones, and the inflammation is of the same scrofulous character as that which attacks other bones at an early period of life. The symptoms follow the same slow and progressive course as in hip disease. At first the patient complains of pain in the lumbar region; in some cases the pain is seated quite close to the hip, and is referred to that joint; or it may show itself from the commencement in the knee—a circumstance very likely to deceive the practitioner, and induce him to refer the symptom to disease of the hip-joint. Lameness soon follows, and the symptoms, which, as you perceive, are the same as those observed at the commencement of hip disease, gradually increase in intensity. But this is not all; they soon give rise to other symptoms also, analogous to the effects produced by disease of the hip. Thus it will be observed the patient avoids lying on the affected side; the nates are flattened and altered in shape; the muscles of the limb become wasted; the patient throws his weight on the sound limb; the affected one is apparently elongated, from depression of the pelvis on the affected side; the knee is slightly flexed, and the point of the foot directed either forwards or slightly outwards. As the disease progresses, the symptoms increase, and the elongation of the limb is followed by shortening, which adds another remarkable point of resemblance between this disease and true coxalgia. If not arrested at this stage, abscesses form in the neighborhood of the posterior spinous process of the ilium,

and open either on the back of the thigh or in the groin; the suppuration becomes abundant, and the patient is worn out with hectic. This unfavorable termination is, however, more rare than in hip disease, for the progress of the malady is arrested, or imperfect anchylosis takes place between the opposite surfaces of the iliac and sacral bones.

From this account, you will perceive many points of resemblance between the two affections: we have in both pain and lameness at the commencement; we have the same, or nearly the same, appearances of the nates and limb, the same position, the same lengthening followed by shortening, and the same formation of fistulous abscesses about the upper part of the thigh. Great attention will therefore be required to distinguish inflammation of the sacro-iliac symphysis in a young subject from hip disease, not only at its commencement, but in subsequent stages. The first point to which attention should be directed is the precise seat of the pain. In hip disease, the pain of the joint, when it exists, is most severe in the groin, or just behind the trochanter major; in the other affection, the pain is chiefly developed by making pressure transversely near the posterior superior spinous process of the ilium. When tumefaction exists, its seat is also different. In hip disease, the fullness is at first perceptible either in front of the joint or immediately behind the trochanter major; in chronic inflammation of the sacro-iliac symphysis, we never discover any tumefaction in the groin, but the puffiness or swelling is always seated over the posterior spine of the ilium. In hip disease, the joint soon becomes fixed, and any attempted motion excites intense pain, whereas the thigh can be freely moved while the patient is lying down, in cases of sacro-iliac affections. The limb, in cases of this latter disease, can be extended without much difficulty; and although the patient may walk very lame, he puts the whole of the foot on the ground; while, in hip disease, the toes only touch the ground, and the heel is elevated. Finally, the shortening of the limb in sacro-iliac affections is always apparent, not real; but in hip disease, the shortening depends on destruction of the acetabulum and head of the femur, or on dislocation of the dorsum ilii; it is real, instead of being apparent.

Formation of matter about the region of the groin or hip, accompanied by pain and difficulty of motion, might lead to the inference of disease of the hip joint in its third stage, and cases are recorded where psoas abscess has been mistaken for it; but attention to the following points will always enable you to distinguish the one complaint from the other. In psoas abscess the pain commences in the lumbar region, and is not felt in the groin until the disease has made considerable progress. Rotation outwards of the limb greatly increases the pain whereas the reverse is noticed in cases of diseased hip-joint; the various positions of

the limb, its elongation, shortening, &c., which characterize morbus coxarius, are not observed in psoas abscess, and the peculiar effect of position, of coughing, &c., in increasing the swelling or discharge of matter depending on psoas abscess, is altogether absent in cases of hip disease.

There are still to be noticed two series of cases, one of which may simulate generally disease of the hip, while the other only resembles the third stage of that affection. The complaints to which I now allude are a peculiar shortening of the limb, produced by injury to the hip-joint, and chronic rheumatism, or, as it has been otherwise called, morbus coxæ senilis. It was formerly supposed that all cases of real shortening of the limb depended either on the hip disease which I have thus described, on fracture within the capsule, or on chronic rheumatism. But Mr. Gulliver has described, in the *Edinburg Medical and Surgical Journal*, (No. 128, vol. xlvi. p. 97,) some cases of shortening of the neck of the thigh-bone, depending upon other causes, and which are particularly worthy of attention. This peculiar lesion has been hitherto attributed either to fracture within the joint, or to the natural effects of old age; but Mr. Gulliver has clearly shown that it may occur in young subjects, as the result of injury to the joint, and under circumstances which give considerable resemblance to the latter stage of hip disease. The history of these cases, as related by Mr. Gulliver, may be comprised in a few words. A young man receives an injury to the hip, from a fall probably, and immediately afterwards complains of pain in the joint. The pain is not very severe, but it continues, and lameness supervenes; these symptoms may go on for many months, the thigh wasting, and the limb slowly shortening, until it becomes perhaps two or three inches shorter than the sound limb.

Cases of the kind described by Mr. Gulliver bear a close resemblance to hip disease in its earlier stage, and at the onset cannot well be distinguished from it; but as the malady progresses the differential signs soon become evident, and a little attention will enable us to discover them. Although the limb be a couple of inches shorter than the sound one, and although the shortening be found to depend on organic lesion, the motions of the joint are perfectly free. Now this could never occur in ordinary hip disease, and the reason why it happens in the cases now alluded to is that the synovial membrane and cartilages are quite free from disease. For the same reason also there is little or no pain produced when the opposite surfaces of the joint are brought into contact by pressing the head of the femur against the acetabulum; there is no tumefaction about the joint, abscesses never form, and the patient remains free from constitutional disturbance of any kind, although the limb is continually getting shorter and the lameness increasing until it becomes incurable. All these

circumstances are incompatible with the notion of chronic hip disease, such as I have described it to you; and even at the earliest stage, while the pain and lameness are considerable, we cannot detect on examination any signs of inflammation going on within the joint. I should also mention that the patient, though he limps, puts the foot flat on the ground—another point of distinction between this disease and morbus coxarius.

A short account of the pathology of these interesting cases will enable you to understand better the symptoms by which they are accompanied. The synovial capsule of the joint, and the articular cartilages, are quite healthy; but the neck of the thigh bone is much shorter than is natural, and forms nearly a right angle with the shaft; or the neck may be removed altogether by absorption, while the head is flattened and expanded. On dividing the parts, the shell of the neck if any remains is found condensed, and, in one case, the cancelli contained caseous matter. Sometimes the acetabulum is widened and remarkably shallow, corresponding to the alteration of shape in the head of the bone. These cases have been assimilated to the examples of interstitial absorption of the neck of the femur which takes place as a natural consequence of old age; but, in my opinion, they have a different origin, and should be referred to absorption—the result of osteitis without any tendency to suppuration.

Analogous, in outward appearance at least, to the foregoing affection, is the chronic rheumatism of the hip-joint, which has been well described by Mr. Robert Smith and Mr. Adams of Dublin. This disease generally affects patients above fifty years of age, though it may occur at an earlier period of life. Its pathological characters are peculiar. The cartilage of the head of the femur is more or less destroyed, not by ulceration, but by a process which seems analogous to interstitial absorption; it looks, in fact, as if it had been worn away by the effects of long continued friction; and the points at which the cartilage is most extensively removed correspond to this notion of wearing down or *usure*, as the French surgeons term it. The neck of the femur is also shortened to a greater or lesser degree, and sometimes so much so that the change produced bears a strong resemblance to fracture of the neck of the bone within the capsule. The os femoris is expanded and flattened, while the acetabulum, which is likewise deprived of its cartilage, is rendered wider and deeper than natural. When the disease has continued for any length of time, the round ligament disappears, and the osseous surfaces, from which the cartilages have been removed, are covered by the porcelain-like incrustation, which I have described in a former lecture; the synovial membrane is not diseased, nor has suppuration ever existed, I believe, within or outside the joint, but the capsular ligament is generally thickened. Many of the symptoms

of this disease are common to it, and to morbus coxarius, and hence the necessity of discriminating between them.

The patient first complains of pain about the joint, increased whenever he throws his weight on the affected limb; the limb itself is apparently shortened to the extent of two or three inches; and this, of course, produces considerable lameness, the limb and foot being rotated outwards. The trochanter major is more prominent than in the natural state, the muscles of the thigh are wasted, but not so those of the calf of the leg, according to Mr. Adams at least. The nates of the affected side are quite flat, and no trace exists of the lower fold of the glutæus. Here we have several of the symptoms which attend morbus coxarius; but the points of difference are sufficiently numerous to render our diagnosis easy. In the first place, if you make accurate inquiries touching the history of the case, you will generally discover that the patient experiences some rheumatic pains in other parts of the body, or has previously labored under an attack of rheumatism—in a word, that he is of the rheumatic diathesis. In the next place you will learn, that the pain and stiffness of the joint are relieved by gentle exercise; when the patient has walked about for a short time, the movements of the joint become somewhat more free—a circumstance quite the reverse of what takes place in hip disease, accompanied by shortening, when indeed the patient is totally incapable of walking at all The pain is not increased by pressing the head of the bone against the acetabulum, and this is a distinctive sign of much value. Finally, on careful examination, you will find that a great deal of the shortening of the limb is apparent, not real; the affected limb may appear to be three inches shorter than the sound one, yet on making accurate measurements of the limb in the manner I have already described, you will discover that two-thirds of the shortening, at least, depend on altered positions of the limb and pelvis. In addition to all this, the constitutional disturbance of true hip disease is never present, nor has the rheumatic affection any tendency to excite the suppurative inflammation which so frequently accompanies the later stages of morbus coxarius. I must, however, observe, that the shortening of the limb in rheumatic cases, which I have just described to you, does not always depend on shortening of the neck of the thigh bone, and deepening of the acetabulum; it may be produced by actual dislocation on the dorsum ilii, and thus is added another remarkable point of resemblance between rheumatic affections of the joint and the ordinary disease.

I have still to consider the treatment of chronic hip disease, but a great deal appertaining to this part of the subject has been already noticed; I shall therefore conclude with a few additional remarks on certain points of treatment which were not sufficiently developed in my preceding lectures. In them I confined my ac-

count of treatment to scrofulous affections, what I shall now add is intended to apply to cases of hip disease occurring in persons who are not scrofulous. When speaking of scrofulous affections of joints, I expressed an opinion unfavorable to the use of active antiphlogistic measures in such cases, but this does not apply to hip disease in strong subjects, who present no signs of a scrofulous habit. Here, blood-letting must be employed, or at least blood must be taken by leeches or cupping glasses from the neighborhood of the joint. For the quantity of blood to be extracted, no fixed rule can be given; you must have regard to the strength and constitutional powers of the patient, his age, the amount of pain, &c., and you must regulate your conduct accordingly. At an early stage of the disease, and in the cases now spoken of, the best general treatment consists in administering calomel, combined with opium, until the mouth is affected. The disease is often arrested as soon as the peculiar effects of mercury manifest themselves; but some patients, as is well known, are unable to bear this powerful remedy, which irritates their bowels, and if continued, may excite dysenteric inflammation of the intestinal mucous membrane. Hence, if pain with tenesmus and scanty mucous stools be observed, an attempt should be made to allay the irritation by opiate injections, and, if these fail, the use of mercury must be immediately abandoned. Counter-irritation may be employed more freely, and at an earlier period in these cases than in scrofulous affections of the hip-joint. When the inflammation has been somewhat subdued by local blood-letting, I generally employ, with advantage, a succession of blisters over the region of the hip. In the second stage, also, counter-irritation must be continued. Some surgeons prefer blisters, while others have recourse to caustic issues behind the trochanter major. When the disease is evidently confined to the osseous tissues, much benefit has been derived from cauterization by means of moxas or the actual cautery. Should any severe inflammatory symptoms appear, we must have recourse to antiphlogistic remedies, and administer purgatives, combined with saline and antimonial medicines. In the cases now alluded to, absolute rest may be enjoined without any great danger of injury to the general health; and hence the patient should be confined to his couch, while the whole limb is kept immovable by a proper apparatus. With respect to other points of treatment, I do not know that anything of importance remains to be noticed. During the third stage of the disease, the same method is applicable to all classes of patients.—*London Lancet.*

London Items.—Dr. Watson is not now connected with any hospital, but is in the enjoyment of a fine practice in the "West End." He never visits patients in the "city." In stature, he is quite imposing. Upon an immense pair of shoulders reposes an immense head, with a prodigious anterior development. Added to these imposing features is an air of the greatest gravity. His measured and deep-toned words issue from his larynx like the low, solemn notes of some huge church organ. He seems almost incapable of excitement.

Sir Benjamin Brodie is the antipodes, complete of Dr. Watson. I had labored under the silly but not very singular notion that Sir Benjamin was a man of six feet one or two inches, with massive shoulders, and everything else in keeping, whereas in fact he is about five feet six inches, and weighs not more than 120 lbs. While, however, his stature or "bodily presence is contemptible," his eye has something of genius in it. His face is rather aquiline, and exhibits much activity and vivacity of expression. He speaks freely and fluently, and seems to have paid some attention to politics as well as to surgery. From the activity of mind and body which he possesses, it is to be hoped he will yet long remain an ornament to the profession. His practice is chiefly, I believe, of a consulting character.

Feeling some interest in the subject of *Porter*, finding it often proper to use it myself in the summer, and frequently prescribing it for others, and moreover having heard such disgusting accounts of the mode of brewing in London, I concluded to visit one of these establishments. I was very politely shown over the entire establishment of Messrs. Read & Co., and was gratified to find that there is no truth whatever in the common opinion, (so common that it has gotten into medical books of high authority,) that the London porter is made from the foulest water of the filthy docks of that great city. One of the agents informed me that many years ago, before the Thames was the common sewer of the city, that it is probable those breweries near the river procured the necessary amount of water from thence, but that such was not the case now. The water at Messrs. Read & Co's. establishment is procured by means of two wells between 200 and 300 feet deep, passing through a thick stratum of chalk, in order to avoid the city drainage. From these wells the water is thrown by steam to the top of the immense establishment, whose tanks receive it, and from whence it is distributed to the "cooling rooms," &c. This water is pure and delightful.—*Stethoscope.*